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AMERICAN ANTIQUITY

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A QUARTER CENTURY OF GROWTH IN AMERICAN ARCHAEOLOGY

FREDERICK JOHNSON

ABSTRACT

To introduce papers presented at a celebration of the 25th annual meeting of The Society for American Archaeology the development of New World archaeology is very broadly and briefly summarized. Expanding knowledge of culture areas and taxonomic systems is traced. The development of salvage archaeology in reservoir areas, along roads and oil pipelines is mentioned. There is an account of the development of various types of relative chronologies in the several areas and comment upon the significance of the introduction of radiocarbon dates. Special mention is made of the possible solution of the question of correlating Mayan and Julian calendars.

IT IS IMPOSSIBLE to fix upon a single date as the most important one in the development of American archaeology. However, the decade of the 1930's saw the beginnings of many trends which are very significant today. The founding of the Society for American Archaeology, in 1935, was an event symbolic of the times. In 1935, there was great need for an organization to represent American archaeologists in numerous ways and to provide an outlet for their work.

The Program Committee in charge of the scientific sessions for the 1960 annual meeting decided that a fitting celebration of the Society's 25th anniversary was to devote a day to an evaluation of the progress which had been made during the past quarter century. The grim threat of the customary annual dinner was removed when Geoffrey Bushnell of Cambridge University consented to speak to us of European opinions concerning American archaeology.

The Program Committee is deeply indebted to all participants in this endeavor. Their friendly cooperation was the reason for the success of the session at which they read papers, and their prompt contribution of their essays has made this publication possible.* We are especially grateful to Helge Larsen of the National Museum of Denmark and to Bushnell not only for their contributions, but for their willingness to undertake the journey to America, so honoring us by their presence. We also gratefully acknowledge the generosity of the Wenner-Gren Foundation which provided a subvention to meet the most pressing of the expenses of the session.

Time, space, and the consideration of the patience of listeners and readers led the committee to suggest that authors restrict their discussions to trends of development in archaeology which could be identified in the several areas of the New World. American archaeology has grown on a broad front and numerous principles are of universal significance. The accompanying essays demonstrate that the real source of archaeological progress is close to the ground, so to speak, and that the nature and status of this growth vary from area to area. The emphasis in these essays is upon the local scene. A few brief and generalized comments serve as a background for these regional appraisals.

American archaeologists, in thinking of the "field," keep in mind, almost subconsciously, the fact of the size of the two continents and the extreme variation in environment and topography which confronted prehistoric man from the time of his first arrival until modern history caught up with him. Because of this, and for various reasons which are largely geographic in nature, it has been customary to divide the New World into areas. The size and character of such areas have depended on the status of archaeological knowledge and the requirements of problems at hand. In 1935, it was easier to determine a boundary between the East and the Southeast and to separate both the latter from the Middle West than it is today. Then, the Plains were a wide and amorphous province and the Southwest such a circumscribed unit that much archaeological work was done without recognition of research elsewhere. At the same time, the West Coast was hardly known outside

* [The Editor expresses his appreciation to Frederick Johnson, chairman of the symposium, for assembling these papers and preparing them for publication. The papers, which were presented May 6, 1960, at New Haven, Connecticut, appear on pages 7–70 of this issue.]

its own limits. The vast area now known as Nuclear America had been the subject of very few successful attempts at broad areal interpretation, and one was forced to organize one's thinking in terms of the archaeology of Mexico, Central America, Peru, and other restricted, often modern, political subdivisions.

Analogous areas have equal if not more substantial validity today. Increase in knowledge has blurred the older boundaries, but the nature of such areas is much more clear than it was. Some units have an integrity which depends as much on details of the ecology as upon distribution of cultural characteristics. Growing knowledge of inter-relationships among prehistoric people and the character of their culture has resulted in revision of ideas concerning geographic factors in human prehistory, so that areas do not limit problems in the manner they sometimes did in the past.

In 1913, Dixon found that in each region of the West Coast he had tested there was but one culture type and that the earlier expressions differed in some respects from the later (Dixon 1913). Also, he recognized the similarity between the prehistoric and historic cultures which continuity is such a notable feature of aboriginal life in the continent west of the Rockies. The East he found a great contrast because of its complexities, and as president of the American Anthropological Association, he belabored eastern archaeologists for investigations which were "woefully haphazard and uncoordinated and the recorded data often sadly insufficient; published reports have too frequently been unsystematic and incomplete; and there has been too little indication of a reasoned formulation of definite problems, with the attempt to solve them by logical and systematic methods" (p. 563). He selected the distribution of Siouian tribes as an example. This gave him an opportunity to condemn archaeological work in the Ohio Valley. With considerable justification, this near diatribe was aimed at unnamed individuals of whose work Dixon heartily disapproved. In a splendidly choleric mood, he ended by writing of the "probable advantage" of systematic investigations in a manner "which rests firmly on an ethnological basis. The time is past," he propounded, "when our major interest was the specimen . . . We are today concerned with the relations of things, with the wheres and whys and the hows; in finding the explanation of the arts and customs of historic times in the remnants which have been left us from the prehistoric; in tracing step by step the wanderings of tribes and peoples beyond history, beyond tradition; in attempting to reconstruct the life of the past from its all too scanty remains" (p. 565). Commentaries on his righteous wrath by W. H. Holmes, George Grant MacCurdy, and Bertold Laufer tempered the insinuations against archaeology as a discipline without detracting from the implied sins in the Ohio Valley. This was all appropriate at the time; it was followed the next year by a series of papers in the *American Anthropologist* under a general title, "Facts and Problems of North American Anthropology." In part, also, this latter was a dress rehearsal of Wissler's culture area theory and the ensuing debate with Dixon regarding problems of culture diffusion, independent invention, and so on.

Twenty-odd years later, that is, in 1935, it is likely that Dixon's criticisms would have had to differ in some important respects. American archaeology had become firmly established as a sector of anthropological research. Questions concerning "relationships of things, the wheres and the hows and the whys" were actively under study. The direct-historical approach which Dixon advocated was a common objective of archaeological work in many areas. With the possible exception of a large proportion of the work in Nuclear America, a great many archaeologists were attempting to connect late prehistoric sites with tribes of Indians and sometimes, even linguistic enclaves. Dixon would have been gratified to learn that the question of the distribution of the prehistoric ancestors of the Sioux was being closely and profitably examined.

There was greater concern, perhaps, for other kinds of questions. Developing interpretations led to consideration of data pertinent to anthropological theory. Archaeology had been completely divorced from the business of collecting curios and the stigma of antiquarianism had practically disappeared. Perhaps the greatest single event which was responsible for cementing the foundation of contemporary archaeology was the initiation of numerous large relief projects in almost all sections of the country. Mound sites, shell heaps, town sites, and large caves were extensively excavated and the material analyzed. Especially in the Southeast, the Mississippi Valley, and the Plains this work brought to light great quantities of specimens and information, some of it quite unexpected. Large scale excavation made necessary expanded and im-

proved field methods and techniques of recording and analysis. The most significant element in this is the character of the men who were in charge of this work. The younger generation in large part took over from the older one without being restricted unduly by them. The work of this group in the field and laboratory was enhanced by concurrent and sometimes subsequent academic training. Much of the progress since 1935 stems from the vitality, ingenuity, and imagination of this dedicated generation of archaeologists. This was true particularly in the United States. There was little or no comparable effect in the Arctic where concentrated archaeological research maintained until the 1940's at least, a steady productive course pretty much after the European tradition. North American archaeologists joined their Latin American colleagues in Nuclear America resulting in a diffusion of appropriate methodology and technique.

With the rapid expansion of archaeology, methods of organizing and analyzing the data shortly became of major interest. The midwest taxonomic system brought some order out of a chaotic situation existing in Eastern North America. This originally descriptive system has been almost completely revised as accumulating knowledge has permitted the introduction of chronology, the search for origins and cultural metamorphosis. In the Plains, however, initial systems based on relative chronology and a sequence of phases of local significance were proposed at the beginning of this period, and elements of the midwest system employed where feasible. Of late, research is approaching the stage where developmental considerations will require consolidation and expansion of a combination of systems. The Southwest has had an analogous history in which chronological and descriptive systems have sprung from alignments based on form, technique, and function. Now the tendency toward identification of broader cultural entities is growing. The Far West is of interest because extensive or area-wide culture classifications have never been generally adopted. The prevailing conditions have favored identification of a myriad of "complexes" which can be included in local frames of reference based on chronology, geology, and ecology as the situation demands. It is likely that the recency of much of the archaeological data has been partly responsible for this dissatisfaction with systems in vogue in other areas and a tendency to postpone generation of some scheme until better ideas come to the fore. In this region, ecological factors have been emphasized for many years and much productive research has been accomplished by close collaboration among archaeologists, biologists, and other scientists. There is the possibility that as research continues a special form facilitating syntheses may well arise. In Nuclear America earlier classifications were largely chronological and local in extent. Various schemes have been put forward in attempts to organize chronologically the developing culture configurations including the expansion of community life. Some of these schemes are controversial but out of the continuing debate it can be seen that chronological considerations will probably be separated from questions of culture development. Discussions of problems in Nuclear America, having given rise to ideas such as "tradition" and "horizon style," are deeply affecting the thinking concerning less complex cultures and communities in North America.

Salvage archaeology has come to be treated almost as a separate branch of archaeology. This habit probably arises from accompanying administrative procedures. There is no other justification. There is not, and cannot be, any difference in the methods or the results of research based on salvage of archaeological remains compared to research which is not urgent. A Federal project designed to excavate a representative portion and otherwise to record archaeological remains in government-sponsored multi-purpose reservoirs commenced with a shamefully limited project in the Missouri Valley in 1945. This was very shortly expanded to include all areas in the United States. Administration of the Federal program began under an agreement between the National Park Service and the Smithsonian Institution. This has grown into the Inter-Agency Archeological Salvage Program which centers in the National Park Service and includes the Smithsonian Institution, the Corps of Engineers, the Bureau of Reclamation, and appropriate sections of the Federal Power Commission and the Bureau of Public Roads. Funds to support this conservation of archaeological data are provided from the Federal budget. An extremely important part of the work is being done by numerous state and local institutions. Some years ago, a system of contracting for excavation, including completion of a report, was initiated by the National Park Service. In a

number of instances, the development of a salvage program has seen the organization and consolidation of archaeological work in local institutions and even the origination or expansion of departments of anthropology. The Committee for the Recovery of Archaeological Remains represented the archaeological profession in the initial organization of the program and continues actively to advise those responsible for it. This committee has also supported the work of a Committee on Highway Salvage established to encourage conservation of archaeological remains threatened by construction of highways, particularly those sponsored by the Federal Bureau of Public Roads. Success of this venture, especially in the Southwest, is recorded by significant published reports. Realization of the need for archaeological salvage has also spread to private industries, notably oil companies constructing pipelines. Further expansion through the offices of the Federal Power Commission affects construction of private reservoirs.

Archaeological salvage as a Federal program differs from federally sponsored archaeology accomplished under W.P.A. and similar relief programs which grew out of the depression. Research destined to salvage materials takes place where reservoirs or other works are being constructed regardless of the proximity of the labor supply and other unrelated factors. As a result, a number of areas have been investigated which, barring Federal developments, would not have been touched for years. Had it not been for this research, archaeologists would have gone on working away at specific and limited problems perpetually lamenting the fact that data from some areas were not available. Salvage projects in a number of instances and in many parts of the country have made it necessary to revise and broaden problems. This kind of stimulation has not always been popular because it may be disturbing, but on the whole the net gain seems to be a large increase in both the quantity and quality of basic data and working hypotheses.

Salvage archaeology which in effect is archaeological work on a large scale and accomplished in the shortest time possible has been responsible for improvement in technology and methodology. During the past 15 years, systems of survey and testing have expanded to a point where they produce much useful data. Use of earth-moving machinery has increased the area which can be uncovered in a given amount of time with a minimum expenditure for labor. This also provides an opportunity, not possible in the days of hand work, to see the relationship of details in a large area of a site. The knowledge so gained permits a much more comprehensive understanding of the prehistoric community it supported.

Twenty-five years ago, chronological problems were paramount and they have continued to be of major significance. Numerous attempts have been made to estimate the time involved in developmental sequences. These estimates have met with varying degrees of success in the several areas, but each one has been transitory. Practically all have been pegged to opinions which at best were based on circumstantial evidence. Relative chronologies have been tied together in rickety structures. These kinds of devices were necessary and useful but they became less trustworthy as they were expanded beyond the confines of the areas in which they originated. Chronological relationships between events in the East, the Middle West, and the Plains were difficult to comprehend because of varying estimates of time. The same was true of more obvious and direct connections between the Plains and the Southwest. Postulation of the age of various complexes became almost an indoor sport, especially among eastern archaeologists during the 1940's. They even invented the horrid expression "guesstimate." Unfortunately, practitioners of the art found with disturbing frequency that they had not placed their bets on the right number and there was constant rearrangement of the temporal assignment of pottery types, cultural units, or whatever category might be under consideration. This maelstrom was profoundly modified by the introduction of radiocarbon dating.

In the Southwest dendrochronology had, by 1935, commenced producing tree-ring dates which could be used to determine the age of archaeological deposits ranging back to shortly before the beginning of the Christian era. The establishment of dendrochronology as a valid system of dating resulted in modification of the chronological framework. This was responsible for now forgotten pangs and heartbreaks among archaeologists because the estimates of ages in relative chronologies based on assumptions of the rate of culture change frequently differed significantly from dates derived from counting tree-rings. This situation is rather amusingly similar to the uncertainties, misconceptions, and face-savings which, in the early 1950's, accom-

panied the introduction of radiocarbon dates. An important attempt was made to expand dendrochronology into the Mississippi Valley. In spite of success at the Kincaid site, the endeavor was found to be too costly and difficult to be continued. Louis Giddings' extension of the technique into Alaska in the 1940's has not been sufficiently recognized (Giddings 1952). Working with inadequate assistance he recorded a growth series permitting determination of dates extending back one thousand years or more.

The debate over the correlation of the Maya calendar with the Christian one which had been raging for more than a decade previous to the founding of the Society continued. Since these papers were prepared there has been published a very significant analysis of the correlation problem and its possible solution by radiocarbon dating. Further comment will be made in another place. The Maya calendar did provide a basis for estimating time but it was never truly satisfactory for there was the possibility of an error which could amount to 260 years, or some other period of unknown but significant length, depending upon the final determination of the correlation. This rendered uncertain ideas concerning the development and spread of pottery types and the like. Sequences based on several possible correlations and modifications of them have been postulated and seriously and profitably debated, but positive conclusions have been thwarted. It may be that the end of this uncertainty is very near.

Useful though they were, all these systems of determining time had serious limitations. They were local in their coverage and the range of time was restricted to less than two thousand years. The discovery of ancient cultures, the paleo-Indian for example, stimulated the trend toward adaptation of geological time estimates for archaeological purposes. These are notoriously generalities, the accuracy of which vary often by as much as plus or minus 30 per cent. Wide and sparse distribution of ancient sites led to a number of discrepancies in estimates and especially a "gap" of several thousand years between the paleo-Indian and the better known, more recent Indian cultures. During the past few years this "gap" has been bridged. To do this required a revision of ideas concerning chronology. The broad search has been materially aided by increasing numbers of excavations, many in Federal reservoir areas requiring salvage of the data. All these operations have brought to light complexes which when dated by radiocarbon indicate a long and continuous occupation of North, Central, and South America. There has been an accompanying expansion of ideas concerning the origin of the American Indian. Twenty-five years ago an hypothesis that man entered the hemisphere by crossing a land bridge at Bering Strait in interstadial times was radical and barely tenable. Now there is a ground swell of opinion, not yet subject to proof, that the first migration may have been earlier, perhaps even during interglacial times. Furthermore, there is the suggestion that what has been called paleo-Indian may be strictly American in origin and that the original newcomer to the hemisphere may have brought a primitive, or at least, simple culture with him. In much later periods the possibility of a multiple origin for various culture traits, including pottery, has become a suggestion requiring serious investigation.

The introduction of radiocarbon dating in 1948, clarified a great many chronological problems, even if it has not yet solved them. This method of dating has produced a framework upon which may be hung events during the past 40,000 years, and in certain instances, 60,000 or even 70,000 years. Radiocarbon dates are numbers which indicate the age of the sample assayed within a given range of error. One of the most valuable properties of the dates is the fact that all of them are determined by a single method and dates all over the world are comparable. The ability to see what was going on in the several areas at a given time has been a vital factor in crystallizing ideas concerning prehistoric cultural development. This may become the greatest single contribution made by the method. Of major current significance is the revision of previous estimates of lengths of time required for the development of numerous sequences. Complexes, the age of which was formerly measured in hundreds of years, are now measured in millennia. It has been shown that the people often called paleo-Indian responsible for fluted points lived about 7000 B.C. and, surprisingly enough, that other peoples, for example those who produced the "Desert Cultures" probably existed at about the same time. Radiocarbon dates of younger complexes have set our knowledge in order, for example, they have demonstrated that Adena overlapped Hopewell in the Middle West.

In Nuclear America, radiocarbon dating provided a chronology for Peru which is useful despite some controversy. In the Maya area radiocarbon dates created an initial confusion comparable to that which followed their introduction in other areas. Pottery sequences and the distribution of various aspects of the culture were trending toward the final adoption of the Goodman-Martínez-Thompson correlation (11.16.0.0.0) of the Maya and Julian calendars. Radiocarbon dates began to indicate the possibility that an earlier correlation might be more accurate. Measurements published in 1951, 1955, and 1959 (Kulp, Feely, and Tryon 1951; Libby 1955; Broecker, Olson, and Bird 1959) appeared as strong support of the Spinden correlation (12.9.0.0.0). In October 1960 (Satterthwaite and Ralph 1960) however, an impressive analysis of a number of samples from Tikal by Elizabeth Ralph corrects the figures and indicates that probably the Goodman-Martínez-Thompson correlation is closest to the true value. Aside from its contribution to the correlation problem, this article is the best demonstration yet published of the proper manner to determine the age of an event by use of an adequate number of carefully collected samples, proper statistical procedure, and sound judgment.

The papers which follow comprise an account of the maturing of American archaeology. It is rapidly approaching the status of a well-rounded vigorous field of scholarly investigation. The body of data now covers the whole range from the remains of simple and rudimentary cultures to the ruins of highly complex cities and nations of Nuclear America. There are serious lacunae in the data which urgently require filling. Also, there is much to be learned through the refinement, description, and analysis of these data. As will be shown, the tools for this are at hand. Excavation methods, recording, and analysis have been developed to a point where they can be adapted to any problem. Statistical procedures are at hand to augment simple counting. Perhaps the most significant progress is seen in what Willey has called "the growing interest in culture process." This is most difficult for archaeology to deal with and complete success cannot be assured. However, attempts are part of much of contemporary archaeological research, and indications for the next 25 years are that the progress now well commenced will continue and grow so to produce a wealth of new, well-founded, and significant interpretations of prehistory in the New World.

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R. S. Peabody Foundation
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ARCHAEOLOGY IN THE ARCTIC, 1935–60 *

HELGE LARSEN

ABSTRACT

Archaeological activities in the North American Arctic are reviewed in terms of the sequences and cultures which have been identified in three major culture areas: the coastal zone from St. Lawrence Island to eastern Greenland, the central area around Hudson Bay with extensions to northern Greenland and Newfoundland, and southwestern Alaska including the Aleutian Islands. Three broad culture horizons are suggested for organizing the data for the first two of these areas. The earliest horizon which cannot be surely identified as either Pre-Eskimo or Proto-Eskimo is tentatively called the Denbigh Flint horizon. It is characterized by fine chipping, microcores, micro-blades, burins, and the absence of pottery. These traits occur in the Denbigh Flint and New Mountain complexes and in other assemblages perhaps as far east as the Sarqaq of Greenland. The second or Paleo-Eskimo horizon which also emphasized fine chipping is found in two forms, one with pottery, such as the Choris, Norton, Near Ipiutak, and related cultures in northwestern Canada, and the other without pottery represented by Ipiutak and Dorset. The latest or Neo-Eskimo horizon is the northern maritime tradition of the Okvik to Inugsuk range of cultures. Other approaches to the interpretation of northern archaeology are summarized.

IN THE ARCTIC it is not so much the quantity as the quality of the archaeological finds that characterizes the last 25 years. Most of the large-scale excavations, that yielded the bulk of the material in our museums today, were made before 1935. Though World War II is partly responsible, it is the shortage of manpower that is the main reason why the amount of field work that has been carried out in the Arctic since then, has not been as extensive as could be desired. There has never been more than a handful of archaeologists working in the area, and when they took time off from field work to write up their results, nothing happened in the field. Actually some of us have spent too much time in the field, enticed away from the desk by the numerous unexplored sites, with the unfortunate result that some of the most significant finds still await final publication. A considerable number of short articles and preliminary reports have appeared during the period with which we are dealing, and even if they are better than nothing, they do not replace monographs.

What we need are more young archaeologists to do field work, and I take this opportunity to appeal to the teachers of archaeology to encourage their students to take up Arctic archaeology. It is a field with rich opportunities for young men and women; there are large, unexplored areas, good chances of finding prolific, well-preserved deposits, and many interesting problems waiting to be solved. Arctic archaeology has been making rapid strides, particularly since World War II. New, unexpected discoveries have resulted in constantly changing aspects and created new problems, but at the same time made the study more exciting. I shall comment on the main events in Arctic archaeology, but first a few words about the area and the background for this survey.

The area includes Alaska with the Aleutian Islands (but not southeastern Alaska), the northernmost part of Canada (roughly to the 60th parallel), northeastern Labrador and adjacent parts of Newfoundland, and Greenland. Though most of the area is arctic it reaches into the temperate zone. In prehistoric times it was the home of at least two different peoples, Eskimos including the Aleuts, and Indians. Since far more work has been done in the Eskimo territory than in that of the Indians, this survey will mainly deal with Eskimo archaeology.

THE PREHISTORIC CULTURES

About 1935, when our survey begins, a period of intensive activity in the field was drawing to a close and most archaeologists were back home writing up their results. When these results appeared the existence of three distinct, prehistoric culture areas were generally accepted, one reaching from St. Lawrence Island along the coasts of northern Alaska and Canada to East Greenland, another centering around Hudson Bay but extending into North Greenland and south to Newfoundland, and the third, what de Laguna has called the Pacific Eskimo–Aleut province.

The first is the home of a cultural continuum originally described by Mathiassen (1927) as the Thule culture, a pronounced coastal culture based primarily on sea-mammal hunting including whaling, and further characterized by permanent settlements with solid, semi-subterranean winter-houses, sturdy, functionalistic implements, the use of ground slate and baleen

* Presented in a symposium, Twenty-five Years of American Archaeology, at the 25th Annual Meeting of the Society for American Archaeology, May 6, 1960, New Haven, Connecticut.

and, in the west, crude pottery. Later other cultures were added, the ancestral Birnirk culture in North Alaska (Mason 1930; Ford 1959), the culture sequence Okvik, Old Bering Sea, and Punuk on St. Lawrence Island (Rainey 1941; Geist and Rainey 1936; Collins 1937), and Inugsuk in Greenland (Mathiassen 1930). The age of this coast culture was estimated at about 2000 years, and northeastern Siberia was considered its most likely place of origin.

Although a very limited amount of material was available from the second area, it was obvious that the so-called Dorset culture was quite different from the Thule culture, which occurred in the same area. It is characterized by extensive use of chipped flint implements, generally smaller tools, and the absence of such characteristic Thule traits as whaling, dog traction, bow drill, and ulu. Jenness, who first described the Dorset culture, considered it older than the Thule culture, and several suggestions have been offered regarding its origin (Jenness 1925; Taylor 1959).

In the Pacific Eskimo–Aleut province only one culture, the Kachemak Bay culture, has been thoroughly investigated and described by Frederica de Laguna (1934). It is rather complex though definitely Eskimo in character. Of its three periods the earliest is of "purer Eskimo type" than the following which have a local stamp, partly due to influences from both sides of the Pacific. Chipped stone tools and socketed harpoon heads are gradually being replaced by polished slate implements and barbed dart heads. Similar cultures had been found in Prince Williams Sound (de Laguna 1956), on Kodiak Island (Hrdlička 1944; Heizer 1956), and the Alaska Peninsula (Weyer 1930). The prehistoric cultures of the Aleutian Islands also showed enough resemblance to include them in the same culture province (Jochelson 1925; Hrdlička 1945). The problems are how they are interrelated, how they are related to other Eskimo cultures, and how and where they originated.

Thus, the main tasks in 1935 were (1) to clear up the relationships between the three cultures, and (2) since all three were so advanced, to look for older and more primitive cultures. As we shall see presently Eskimo culture appeared to be much more complex than anticipated; as Jenness (1952: 30) expressed it: "Eskimo archaeology is following the same pattern as atomic research; each fresh discovery opens up new and alluring vistas, but only deepens the principle mystery." The quotation is from a discussion of the Ipiutak culture, the first of a series of discoveries that added to the complexity of Eskimo culture (Larsen and Rainey 1948).

Alaska

Though the Ipiutak culture was first found at Point Hope, Alaska, in an area where Birnirk and Thule cultures prevailed, and though it existed at the same time as Okvik and Old Bering Sea and had several traits in common with them, it does not conform to the pattern characteristic of the Arctic Coast culture. The absence of floats and several other elements connected with hunting in open water indicates that it was not fully adapted to the coast. At the same time it possessed a number of inland traits and had a dual economy based as much on caribou hunting as on sealing and walrus hunting. The Ipiutak culture may therefore be characterized as a coast-inland form of Eskimo culture. It has an older stamp than Okvik and Old Bering Sea, borne out by the predominance of excellently chipped flint implements and the extensive use of side blades; further by the absence of a number of culture elements common in most Eskimo cultures such as the blubber lamp, ulu, bow drill, fishhooks, ground slate, and pottery. In order to distinguish between the Arctic Coast culture and this old-fashioned and in certain respects less developed culture we found it useful to adopt a terminology used first by Steensby (1916) and later by Birket-Smith (1929) and apply the name Neo-Eskimo to the coast culture and Paleo-Eskimo to Ipiutak.

The occurrence of chipped flint implements and other Ipiutak traits elsewhere in the Eskimo territory led us to the assumption of the existence of a widespread Paleo-Eskimo culture complex antedating the Neo-Eskimo culture. At Point Hope itself we found evidence of other manifestations of Paleo-Eskimo culture, which we treated under the name Near Ipiutak. It contains implements that seemed to be more primitive than those from Ipiutak proper, but also elements like ground slate, pottery, and whaling harpoon heads, which we then thought were due to Neo-Eskimo influence and consequently indicated a later age for Near Ipiutak. The Ipiutak culture itself we dated to the first centuries of the Christian era, but we assumed that other Paleo-Eskimos had crossed Bering Strait from Siberia in pre-Christian times.

Giddings' discovery of the Denbigh Flint complex in 1948 marks the beginning of a new epoch in Eskimo archaeology (Giddings 1949 and 1951). It was the first time a culture had been found that was old enough to be considered in connection with the question of the origin of Eskimo culture, and it contains two elements, burins and micro-cores and microblades, which add reality to the previous assumption of a connection between Eskimo culture and epi-paleolithic cultures of the Old World. I shall later return to the question of the relation between the Denbigh Flint complex and the Eskimo culture. The Denbigh Flint complex has also points of resemblance with the paleo-Indians, namely a fluted point and gravers reminiscent of Folsom, and two points, one resembling the Scottsbluff type, the other the Plainview type. A third group of implements consists of arrow points, side blades, harpoon blades, and scrapers made with a chipping technique that equals the finest in the world. One of the most remarkable features is the fine diagonal flaking on the extremely delicate points and side blades, of which many were made from microblades. Another exceptional feature is the use of the tiny burin spalls as a separate tool (Giddings 1956a).

The discovery of the Denbigh Flint complex added several thousand years to the human occupancy of the Eskimo territory. We do not know yet just how old the complex is. According to the latest radiocarbon tests it dates back to at least 2000 B.C., but it may be considerably older (Rainey and Ralph 1959). One of the questions that arose immediately after the discovery was whether the Denbigh Flint complex has any connection with the Eskimo cultures. In my opinion it is much easier to find arguments in favor of a connection than the opposite. In the first place Denbigh-like flint assemblages have been found at the same localities as Eskimo culture over large parts of the Eskimo territory, just as at Cape Denbigh. From the position of the Iyatayet site and the probability that the Denbigh people used harpoons we may infer that they were there for the same purpose as later occupants, namely seal hunting, and that in itself is significant, particularly since Meldgaard has found typical Eskimo harpoon heads in a Denbigh-like assemblage at Igloolik (Birket-Smith 1959, Fig. 3). But the Denbigh people were also caribou hunters. This we know from the presence of arrow heads in the find from Iyatayet, but also from the findings of Denbigh-like materials in the interior of northern Alaska (Solecki 1950 and 1951; Irving 1951, 1953), from the occurrence of true Denbigh artifacts with caribou bones in the Trail Creek caves (Larsen 1951), and in particular from the Engigstciak site, where the bearers of the closely related New Mountain complex were primarily caribou hunters, but also hunted seals (MacNeish 1959). It seems to me that in the Denbigh Flint complex we have another example of a coast-inland culture. This is just one point of similarity with the Ipiutak culture; others are flint implements of the same shape, though the Ipiutak specimens are larger and not so finely chipped, and the occurrence in both cultures of arrowshaft-smoothers with a longitudinal groove and birch-bark containers. Parallels with the Dorset culture are micro-cores and microblades and a burin-like implement, which undoubtedly is a further development of the true burin. This implement, which was identified by de Laguna (1947: 193), and which also occurs in the Ipiutak and Old Bering Sea cultures (Larsen and Rainey 1948, Pl. 47, 16; Collins 1937, Pl. 39, 19–22), is usually of silicified slate and with ground surfaces, but it was used as an ordinary burin. Incidentally, the latest development of the burin, made of metal, may still be in use (Meldgaard 1952: 228). Even if we cannot dissociate the Denbigh Flint complex from the Eskimo cultures, we have not yet solved the problem of the relation between them. The question is, whether we may call the Denbigh Flint complex an early or Proto-Eskimo culture, or a Pre-Eskimo culture. At this stage I think it is wrong to use either term. In the literature the term Pre-Eskimo has often been used in connection with the Denbigh Flint complex, but we have no proof that the Denbigh people were not Eskimos.

In the Iyatayet site there were no traces of people for the next 2000 years or more, but that does not mean that the north was abandoned. Indications of human activity in that period were found in the fill of the Trail Creek caves, and in the last few years these indications have manifested themselves in actual cultures and complexes, to which I shall return later. Here just a few words about the Iyatayet middle layers, or the Norton culture as it is now called (Giddings 1957: 132). According to Giddings (1960: 127) the Norton culture is like Near Ipiutak in most identifiable traits, and if the two cultures turn out to be identical I see no reason

why we should not rename Near Ipiutak, which is a poor name anyway, and call it Norton. This must, however, await the publication of the Norton material from Iyatayet. In contrast to Ipiutak the Norton culture has stone lamps, well-fired, linear and check-stamped pottery, and some ground slate. It is older than Ipiutak; if one may trust the latest published radiocarbon dates the earliest date of Ipiutak should be about A.D. 300, and that of Norton about 300 B.C. (Rainey and Ralph 1959). This means of course that we must revise our first interpretation of the presence of pottery and slate in the Near Ipiutak finds from Point Hope as being elements borrowed from Neo-Eskimo cultures. Before turning to another part of the Arctic I shall just mention, that investigations in the Kuskokwim Bay area in 1948 revealed two different forms of Paleo-Eskimo culture. At Platinum was found an Ipiutak-like assemblage without pottery but with stone lamps, while finds from Chagvan Bay and Nanvak Bay have linear and check-stamped pottery like the Norton culture. They have also traits in common with cultures of the Pacific Eskimo–Aleut province (Larsen 1950).

Greenland

The year 1948 is also a memorable one in Greenland archaeology, because that year two discoveries revealed that Greenland had been inhabited thousands of years earlier than was generally believed. That Dorset people had moved into North Greenland ahead of the Thule people was known, but that people had lived in Peary Land 4000 years ago was a surprise to everybody. Without going into details I shall briefly outline the picture that begins to emerge of Greenland's earliest prehistory. The oldest remains have been found by Eigil Knuth in Peary Land on terraces 12–20 m. above sea level. This culture, which Knuth has called Independence I, has chipped burins, micro-cores and microblades, and side blades like the Denbigh Flint complex, but lacks its very small, finely chipped implements and the paleo-Indian parallels. On lower terraces, 5–6 m. above the sea, Knuth found remains of a different culture, which he calls Independence II, a thousand years younger and reminiscent of one of the earliest stages of the Dorset culture, which I shall mention presently (Knuth 1952, 1954 and 1958). In West Greenland the excavations at Sermermiut, a stratified midden with culture-bearing horizons separated by sterile layers like Iyatayet, revealed three distinct cultures. At the bottom Sarqaq with a flint industry reminiscent of the Alaskan Paleo-Eskimo cultures, but with chipped and partly ground burins not unlike those of the Denbigh Flint complex. Above the Sarqaq culture, which has been dated by radiocarbon at about 800 B.C., was a layer of Dorset culture with the characteristic burin-like implements and mirco-cores and microblades. It represents an earlier stage than the classical Dorset, and has been dated to the first century A.D. The top of the midden was represented by the Thule culture in its Greenland form (Larsen and Meldgaard 1958; Mathiassen 1958; Larsen 1960a, 1960b).

Canada

For many years northeastern Canada had received little attention, but with Collins' and Meldgaard's investigations in 1954 it suddenly became the main point of interest in Arctic archaeology (Collins 1956; Meldgaard 1955, 1958, 1960). Working independently they both discovered older stages of Dorset culture not recognized before, and in both cases in considerable height above and distance from the shoreline. Here, as in Peary Land, there has been an uplift of the land leaving rows of old beaches which, if they have been inhabited, give us the relative age of the cultural remains. At Alarnerk, on Melville Peninsula, Meldgaard was fortunate to find a place with many old beaches, one above the other and several of them with house ruins, middens, and graves. By making excavations at different levels he found successive stages of Dorset culture from 8 to 22 m. above sea level. Though the development seems to have been gradual, he was able to distinguish between five periods, of which the last two correspond to what we hitherto have considered *the* Dorset culture, while Periods I–III were new to science. Characteristic of the earliest are side-notched blades of ground slate, but they disappear in Period III, where they are replaced by similar implements of chipped flint. Burin-like implements occur in all periods, but no true burins were found. At Kapuivik near Igloolik, just north of Alarnerk, Meldgaard found remains of a pre-Dorset culture on terraces 38–52 m. above sea level (Meldgaard 1958). It corresponds in age and inventory to Independence I, though the implements are generally smaller and in that respect more like those of the Denbigh Flint complex. It was here Meldgaard found the toggle harpoon heads mentioned before, one of the

strongest indications that Eskimo culture is much older than generally assumed. The relation between this early culture and the Dorset culture is not yet clear, but Meldgaard does not consider it impossible that Dorset is an outgrowth of it, in which case it seems necessary to assume a strong influence, probably from Indian cultures, in order to explain the sudden occurrence of ground slate implements.

Until the middle fifties we knew practically nothing about the archaeology of the vast areas between Hudson Bay and the Alaskan border, the very region Steensby and Birket-Smith had pointed out as the original home of Eskimo culture. Since then Giddings (1956b), Elmer Harp (1959a, 1959b, 1959c), and particularly MacNeish (1956, 1959a, 1960) have made good this deficiency; the first two by their finds of Sarqaq-like assemblages west of Hudson Bay, and MacNeish by his excavations of the Engigstciak site, 16 miles from the mouth of Firth River, near the Alaskan border. The material from Engigstciak is of the utmost importance to our understanding of the development of the Eskimo culture, but it is so extensive and has so many implications that it is impossible in this brief review to give it the attention it deserves. Instead I have chosen to mention a few facts that fill in gaps in our knowledge. In the first place there are two complexes that apparently antedate the Denbigh Flint complex, both with burins and both associated with extinct animals. The third complex, the New Mountain, seems to be closely related to the Denbigh Flint complex, having similar burins, microblade tools and side blades with ripple-flaking. Thanks to the preservation of organic materials we know that the bearers of the complex hunted caribou, buffalo, elk, Rocky Mountain goat—and seals, in other words a coast-inland culture. Next follow three phases, Firth River, Buckland Hills, and Joe Creek, which seem to fit into the gap between the Denbigh Flint complex and Norton. All of them have pottery, but there is a change in its ornamentation and also in the flint work. Partly ground burins and ground slate begin to appear in the last phase, and it seems as if microblades are gradually being replaced by crescentic and semilunar side blades. In the next phase, Cliff, which MacNeish correlates with Norton and Near Ipiutak, burins and microblades have disappeared. It contains ground slate tools and linear and check-stamped pottery like Norton. The last two of the nine different cultural manifestations represented at Engigstciak are Neo-Eskimo stages corresponding to Western Thule and Tigara at Point Hope.

Back in Alaska, where we started our survey, Giddings has recently conducted some interesting investigations, which are still going on (Giddings 1957, 1960a, 1960b). They began in 1956 with the discovery of large, oval houses at Choris containing among other things linear-stamped pottery, some poorly ground slate, and flint points with diagonal flaking. It seems to be earlier than Norton and contemporary with Joe Creek and the middle layers in the Trail Creek Caves. Of particular interest are Giddings' recent investigations at Cape Krusenstern, where there are many old beaches, one behind the other but all of about the same elevation. The difference in age between the beaches is here given by the distance from the sea rather than by the elevation. In addition to already known cultures, ranging from Western Thule to the Denbigh Flint complex, Giddings has found on these beaches new assemblages. I shall mention only one, the Krusenstern Notched Point assemblage, which occurs between beaches with Choris-like pottery and flints and the Denbigh Flint complex beaches. I mention this specifically because it, among other things, contains large, bifacially chipped blades, which Giddings believes are blades for whaling harpoons. If they are, and if Giddings is right in his estimate of the age of this assemblage as earlier than 1500 B.C., it means that whaling is much older in this area than anticipated. It thus seems that three elements which form an integral part of the Alaskan Neo-Eskimo culture, namely whaling, pottery, and ground slate, are present at a very early date, and other traits might appear when more organic material from early horizons are brought to light. In other words, the stage was being set for the development of the Neo-Eskimo culture as we know it from Alaska.

SUMMARY

Even if the situation looks more complicated today than 25 years ago, the many new discoveries have given us certain lines of direction that may lead us to the solution of at least some of our problems. Disregarding for the time being MacNeish's two earliest complexes, which need more investigation, it seems to me that one may distinguish between three culture horizons or layers in the area I have dealt with here, that is, north and east of Norton Sound. The oldest

horizon, which for the time being, we may call Denbigh until we know if it is Pre-Eskimo or Proto-Eskimo, has the following characteristics: finely chipped flint implements, burins, microcores and microblades, side blades, and the absence of pottery. All or most of these traits have been found in assemblages in northern Alaska, Engigstciak's New Mountain complex, the Igloolik area, and Peary Land. Perhaps Knife River (Giddings 1956b), Dismal Lake (Harp 1959a) and Sarqaq should be included even if at least the latter is different in some respects. The second horizon I would call the Paleo-Eskimo. It is also characterized by extensive use of chipped flint implements and occurs in two forms: one with pottery as in Choris, Norton, Near Ipiutak, and Chagvan Bay in Alaska, and Firth River, Buckland Hills, Joe Creek, and Cliff in Northwest Canada; and one without pottery as Ipiutak and Platinum in Alaska, and the Dorset culture. There is reason to belive that one or more of the Paleo-Eskimo cultures developed out of the older horizon, at least in the West, but probably also in the Hudson Bay area. The third horizon is the Neo-Eskimo culture, which I believe developed out of one or more of the Paleo-Eskimo cultures. This framework does not include all forms of Eskimo culture, for instance not the whole Pacific Eskimo–Aleut province, the origin and relationships of which we know as little about as we did 25 years ago.

The scheme I have just outlined is only one way of interpreting the findings to give an overall picture of the situation today. Another way is to use the concept of tradition which very recently has been discussed by both Giddings (1960a) and MacNeish (1959b). It takes a lot of courage to publish a scheme as far-reaching as MacNeish's at this time. MacNeish must have realized that he stuck his neck out, and that we would all try to make him change parts of the scheme. Although I have a number of suggestions as to changes, on the whole I consider the chart useful in discussions about the relative age and connections between the many assemblages with which we are now operating. Of the 10 traditions suggested by MacNeish, five appear to me to have relations with Eskimo culture. Of these, three may be accepted without comments, namely the Dorset, the North Pacific (mainly the Kachemak Bay culture), and the Aleut traditions. Of the other two I find Arctic Small Tool tradition a good term for Denbigh and related cultures, primarily because it is noncommital regarding Eskimo culture. The term was first used by William Irving in 1957. MacNeish's last or Inuk tradition (Inuit would have been better), is too broad a concept, because it actually comprises two traditions. We may use the terms Choris–Near Ipiutak–Norton tradition for the earlier one and Okvik-Inugsuk or northern maritime tradition for the later, as Collins (1960: 135) has suggested recently; or we may call them the Paleo-Eskimo tradition and the Neo-Eskimo tradition. When I prefer the latter two terms it is neither because of stubbornness nor the desire to conform to the hypotheses of Steensby and Birket-Smith, but only because I find it practical to use terms that serve a double purpose, namely to cover a cultural continuum and at the same time indicate their relative age.

Another approach that has entered the Eskimo literature very recently is the question of diffusion versus migration (Giddings 1960a). I do not deny that some of us, though well aware of the occurrence of trade pieces, may have underestimated the importance of diffusion as a means of distribution of culture elements. However, we must not go to the other extreme and underestimate the significance of migrations. Migrations have taken place in the Arctic ever since the first people moved in more than 4000 years ago. It has occurred to me that perhaps we were misunderstood when we used the word migration in our writings, so let us make that clear first. I think I may say for sure, that no archaeologist, who has worked in the Arctic and is familiar with the habits of the Eskimos, has ever visualized a large body of Eskimos breaking up from their home determined to migrate to some distant place. When Eskimos move it is always in small groups, one or a few families, and usually in order to find new and, they hope, better hunting grounds. We have an excellent example in the Iglulik Eskimo families that in the 1860's broke up from Ponds Inlet and migrated north to North Devon and Ellesmere Land and finally ended up in Thule. Some of them returned, but others stayed and introduced such important culture elements as kayak, bow and arrow, and salmon spear, which had gone out of use at Thule. These elements were added to the culture of the Thule Eskimos as a result of a normal Eskimo migration and not through diffusion. We must not forget that

diffusion presupposes a certain density of population. In the Arctic, and particularly in Canada and Greenland the inhabited regions are often separated by vast empty spaces or uninhabited stretches of coast, which reduces communication and thereby diffusion to a minimum. As an example I may mention that despite the thousands of archaeological specimens that have been excavated in the Thule district, there is not the slightest evidence of connections with West Greenland for a period of 300 years. The distance is too long and the coast too difficult to pass to permit trade and diffusion of culture elements. The cultures that have come to West Greenland from Thule, namely Sarqaq, Dorset, and Thule, must be the results of migrations. Meanwhile, in order to avoid misunderstandings in the future we may adopt the concept of "population spread" suggested by Giddings (1952), which by and large covers what I believe most of us have in mind when we talk about Eskimo migration.

Not enough work has been done in the Indian territory to enable us to form a picture of the development there. The only one who has made an attempt is MacNeish, who has also done most of the digging, and on whose "Speculative Framework" I have based this brief statement (MacNeish 1959b, 1960). It is interesting to note that the Campus site which was excavated in the Indian interior just 25 years ago (Nelson 1937; Rainey 1939), yielded micro-cores, microblades, and burins (Irving 1955). MacNeish suggests that micro-cores and microblades, which since then have been found in many places in the interior, belong to what he calls the Northwest Microblade tradition. It is fairly widespread in time and space. MacNeish believes that it might have influenced the Arctic Small Tool tradition. In addition he sees three earlier and less well-founded traditions. One of these, his Yuma tradition, may have developed in the Plains and spread northwards even into the Eskimo territory where MacNeish, sees it in Campbell's (1959) recently published Kayuk complex from Anaktuvuk Pass. MacNeish's latest tradition, the Denetasiro, is that of the ancestors of the Athabaskans. Its origin is not known, and I fully agree with MacNeish that it is a key problem in northern prehistory. I hope that this and the many other problems that the Arctic offers may attract young, energetic archaeologists, so that we may have the solutions at the 50th anniversary of the Society for American Archaeology, if not sooner.

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NATIONALMUSEET
Copenhagen, Denmark
January, 1961

ANNOUNCEMENT OF THE 27TH ANNUAL MEETING

The 27th Annual Meeting of the Society for American Archaeology will be held in Tucson, Arizona, May 3, 4, 5, 1962. The University of Arizona is the host institution, and all formal sessions of the meeting will be held on the campus. The program will consist of five symposia and as many topical and areal sessions as may be necessary to handle the volume of contributed papers. Organized field trips to archaeological sites in southern Arizona will be available following the meeting. William W. Wasley is Chairman of the Program Committee, and Raymond H. Thompson, Chairman of the Local Arrangements Committee.

The symposia are being arranged around the following topics: Hohokam archaeology in 1962 (plenary session), tree-ring dating throughout the world (sponsored by the Tree-Ring Society), history of metallurgy in Nuclear America, archaeological progress in the Intermediate Area, and problems of administration in emergency archaeology programs. The deadline for all papers is February 1, 1962. Contributors to the symposia must supply completed manuscripts before that date, and all other participants in the program must provide the following information: title, abstract of not more than 100 words, type of slides or other illustrative material, full details on projection equipment necessary, map and artifact display requirements, and any other special needs. The abstracts will be reproduced and distributed with the program. Maximum time allowed for contributed papers will be 20 minutes.

Note that no papers will be accepted for the program of the 27th Annual Meeting after February 1, 1962. Avoid disappointment and difficulties by respecting this deadline and by discussing any potential problems and delays with the Program Committee as soon as possible. Address all communications concerning the program to Wasley, Arizona State Museum, University of Arizona, Tucson, Arizona.

The Pioneer Hotel will serve as headquarters for the meeting. Reservations for rooms must be made directly with hotels and motels in Tucson. Further details on housing will be supplied at a later date.

The 28th Annual Meeting will be held at Boulder, Colorado, May, 1963, with the University of Colorado as host institution.

TWENTY-FIVE YEARS OF EASTERN ARCHAEOLOGY *

WILLIAM G. HAAG

ABSTRACT

Archaeological research in the Eastern United States is reviewed from the period of large-scale excavations sponsored by federal relief agencies to the present period of trend toward interdisciplinary approaches. An early interest was the identification of the archaeological ancestors of historically and ethnographically known tribes. The need for objective and descriptive classifications led to the development of the McKern or Midwestern Taxonomic Method. Recent developments include the use of functional and evolutionary concepts experimentation with statistics to validate analytical and classificatory techniques, discovery of stratigraphic evidence and the perfection of seriational techniques, emphasis on the value of geological and biological data and the combination of these with archaeological information to produce an ecological approach, and attempts at broad culture-historical synthesis. The introduction of radiocarbon dating, which exposed the extremely conservative time estimates of Eastern archaeologists by greatly lengthening the chronologies, is seen as the major contribution of the past 25 years.

THE LAST 25 years have produced a rather revolutionary change in our knowledge of Eastern archaeology. Not only has the gross content of our knowledge increased but the theoretical orientation as well has undergone a marked change. The personalities most active at the beginning of the period have largely faded from the scene by now, their various replacements each acting out his part in his time. The magnitude and content of many notable contributions have been controlled by the intellectual climate of the moment so that some individuals have been recognized appropriately and others have not been so fortunate. In Eastern archaeology this quarter century has been one of growth and maturing, especially in conceptualization and in technological approach to problems.

Twenty-five years ago archaeological research in the East was undergoing a tremendous surge of activity. This was a direct outgrowth of the several federal government work-stimulation programs such as FERA, PWA, WPA, CWA, and CCC. It might be euphemistically argued that the paternalistic government of the era was genuinely interested in conserving archaeological remains threatened by inundation of flood waters behind dams. Actually the motivation was the fact that archaeological projects used over 90% of the allotted relief funds for labor rather than supplies, in contrast to road and building construction. No matter what altruism moved these benefactors of prehistory, Eastern archaeologists look back to this time as the heyday of the discipline.

The subarea in which most excavation was being done in 1935 was the Southeast. Much of this work was continuation of projects of the previous year, projects begun in the Southeast because of its milder winter weather. Also, much was opportunistic; wherever relief labor was available archaeology flourished. There were larger relief rolls in the southern portion of the East than in the northern part. Nevertheless, out of this often haphazard program of excavation came some of the enduring investigations of this half century. Setzler and Ford at Marksville established a new dimension in Hopewellian culture. Kelly and associates at Macon began a study that has developed to monumental proportions. In New York, Ritchie continued detailed investigations of a number of significant sites. Shortly after this time, Webb resumed large-scale excavations along the Tennessee River in north Alabama and in 1937 expanded operations to western Kentucky. Subsequently, other rather extensive investigations in Tennessee, Louisiana, Florida, and elsewhere were carried forward until the demise of federal aid in the face of World War II.

Not all archaeological investigations of the early years of this period were large. In several noteworthy cases individual mounds and middens were specifically sought out and studied. The Peachtree Mound in North Carolina, excavated by Jennings (Setzler and Jennings 1941); the Nowlin Mound in southeast Indiana analyzed by Black (1936); and the Kincaid site in Illinois, worked for several years by University of Chicago personnel (Cole 1951), are examples. In New England, Byers and Johnson (1940) selected small sites most revealing of information, and in Florida an early relief-financed excavation on Canaveral Peninsula put into orbit a new idea of Florida-Antillean relationships (Setzler and Strong 1936). In fact, Eastern archaeologists rather quickly posed the main problems and by 1940 were ready to consider the results of a near-decade of excavation.

* Presented in a symposium, Twenty-five Years of American Archaeology, at the 25th Annual Meeting of the Society for American Archaeology, May 6, 1960, New Haven, Connecticut.

A brief pause in excavation and a look around the East seemed to have revealed the existence of great hiatuses in our knowledge. At least, if we make this assumption, the inspiration may be seen for the several extensive surveys, such as the Lower Mississippi Alluvial Valley Survey of Phillips, Ford, and Griffin (1951) or Willey's Florida Gulf Coast survey (1949). Neither of these surveys was inspired by conservation or salvage, but by a desire to find an empirical basis for archaeological reconstructions in crucial areas.

By 1935 American archaeologists had emerged from an antiquarian orientation to one essentially historical. Some idea of the diversity of new interests developing may be gained from an examination of the roster of persons attending the Conference on Southern Prehistory in Birmingham in December, 1932 (National Research Council 1932). Most of the active participants were archaeologists: Cole, Collins, Guthe, Judd, Moorehead, Stirling, Strong, Walker, and Webb, men already recognized as leaders in various areas of American archaeology. Clark Wissler was Permanent Chairman of the session. The only then-younger archaeologists present were Ford and DeJarnette. Significant however, is the presence of non-archaeologists such as Dellinger, Kniffen, Linton, and Swanton.

It seems warranted in this historical sketch to point out that of the list of notables at the Birmingham conference only W. S. Webb enlarged his activities and identity with Eastern archaeology. Perhaps Cole, to a lesser extent, may be said to be thereafter concerned with Eastern archaeology, but all the others found interests and obligations in other spheres. There never was much of a "Young Turks" movement, for after 1935 Eastern archaeology was developing so rapidly that there seemed ample room for all. Actually, it is most probable that the younger archaeologists largely ignored the older element. Chronology was the interest of the former whereas the attention of the latter was not centered upon any one approach.

If archaeologists of the early thirties had a theoretical frame of reference, it may be easily identified or equated with history. The basic assumption was that the historic tribes were the lineal descendents of the archaeological remains among which they lived. It was held that the threshold of history could be pushed back in time by the proper evaluation of artifacts for uses that might be understood in terms of Eastern ethnography. For example, Webb was a profound student of Eastern history and ethnography. His personal library was filled with such volumes. His early writings reflect this interest and knowledge. By the same token, men from ethnography, notably Swanton, were seeking in the archaeology of the East the answers to problems of ethnic and linguistic relationships.

Despite an absence of Young Turks the names that were significant in 1935 to 1940 were relatively new. Virtually every name of stature today is a product of these times. Some were migrants from other climes, such as the Southwest, but many more were the extended shadows of former active Easternists. Ford took his initial impetus from Collins, Richie carried on from Parker, and Black was irrevocably committed to archaeology by Moorehead. Only a rare individual, such as Griffin, may be an American Athena. Nevertheless, the over-all picture of the times bears little resemblance to the archaeology of the preceding 25 years.

At the close of the Birmingham Conference of 1932, Dellinger voiced the hope that the affair would be often repeated, but under the same aegis it never was. Yet the idea of the "conference," of the free exchange of ideas and findings before publication that had been so successfully used by this time in the Southwest and in the Plains, became an implement of progress in the East. The Indianapolis Conference followed by the Woodland Conference at Chicago was markedly successful and timely. Although the original function of the "conference" is much adumbrated, such recent gatherings as the Archaic Conference and the Radiocarbon Conferences at Andover, the Adena Conference at Lexington, and the annual Southeastern Conferences clearly attest the remaining vigor of the idea. The social aspects of such sessions may well be their most important function, since many problems are resolved immediately and never clutter up the printed page.

Perhaps a singularly useful exercise at the moment would be a cursory examination of the articles on Eastern archaeology in AMERICAN ANTIQUITY in this first 25 years. It is hardly a surprise to find no articles in Volume 1 that are summations or syntheses; most are concerned with site reports. Actually, over the years, the use of the journal for site reports is rather limited, but in early volumes there are

several writers on specific areas, such as Ritchie on the Northeast and Goggin on Florida, among others.

Classification was obviously a necessary interest in the late thirties and the journal reflects it. Elaborate attempts were made to erect all-encompassing schemes for various artifacts. The lack of adoption of these schemes demonstrates the futility of such attempts. Also, the journal reflects the changing concepts about classification and the concern with fundamental theoretical positions.

Another fact discernible from AMERICAN ANTIQUITY articles is the trend away from descriptions of individual artifact types. Only in the early issues are there descriptions and analyses of specific artifacts such as bone points or specialized knives. Of course, it is unfortunately true that pottery is still used in this manner in the East, much as if to prove that the Greeks were right about handmaidens.

It would be a serious omission to fail to mention some of the invaluable monographs that have appeared in other media than AMERICAN ANTIQUITY. Griffin's *Fort Ancient Aspect* (1943) is such a classic. Lewis and Kneberg's *Hiwassee Island* (1946) has certainly inspired emulation. The numerous state organization bulletins and university sponsored publications have become sources of vast stores of information. In contrast, it must be said that Jennings was quite correct in his Cassandran predictions at the Society's annual meeting in Urbana in 1953. Since that time not many pages of the *American Anthropologist* have been devoted to archaeology and none specifically to the East.

In the first 25 to 35 years of this century, the newly established departments of anthropology were often staffed or chaired by Boas-trained students. That Boas was the promulgator of the historical method into American anthropological techniques seems accepted. Although there is no explicit recognition of Boas' contributions to archaeological theory, he initiated and propounded the basic relationships between American ethnography and prehistory. This occurred about the turn of the century; yet it was not until his students, such as Cole and Deuel, or persons he had influenced, such as Swanton, became the most active anthropologists in the Eastern field, that the so-called direct historical method caught hold. All practicing archaeologists must "do history" sometimes, but today only a few think this is the ultimate goal of archaeology. In essence, the historical method demanded the amassing of great quantities of detailed information about any given archaeological manifestation in order to compare it with any other situation. This argued for generalizations — if any were possible — that were based upon empirical data. Archaeologists of the Eastern United States virtually became taxonomists anxious to organize their findings in such a way that each occurrence could be correctly pigeonholed. Taylor (1948: 77) is quite correct when he finds that Webb of Kentucky gradually lost his ethnographic orientation and became one of the more devoted taxonomists. Generally, Eastern "trait lists" made not even a token gesture suggestive of seriation. When seriation finally was used, as exemplified in the Adena trait list, it made just as much logical sense when read from bottom up as top down, since stratigraphy was lacking (Webb and Snow 1945: 213).

In the midst of this happy preoccupation with history, just slightly more than 25 years ago, Radcliffe-Brown appeared. After a short and interrupted stay he left in his wake a dozen students some of whom never again could be called "historical" nor even "archaeological." Miner, Bennett, Neitzel, Jennings, DeJarnette, Orr, MacNeish, Brown, Harrington, Caldwell, and Wilder are well-known names of this congress. Bennett (1943) has been singled out and lauded as one who has gone beyond "mere taxonomy and chronology" (Taylor 1948: 91). It is noteworthy that he was able to discern the functional point of view in the works of several Eastern archaeologists. It is not really remarkable that functionalism should enter into Eastern archaeological writings. It is vastly more astounding that functional studies are so few. Steward and Setzler (1938) pointed out that the preoccupation of archaeologists with artifacts and their arrangement in historical sequence precluded the use of functional classes such as "horticultural implements," "hunting," "household," and so forth. Much of the reluctance to define archaeological remains in functional terms stems from the view that value judgments are "unscientific."

Perhaps it is even more remarkable that there have been so few Eastern archaeologists in the last 25 years who have been committed to an evolutionary approach. In a recent article I argued that several current archaeologists will be judged in the future to be evolutionists

(Haag 1959). Stated simply, evolutionism demands a commitment to the idea that the whole cultural configuration of mankind has undergone change in form and function through time and that these changes have been brought about through causes residing within culture. The histories of individual cultures vary since a variety of factors condition the utilization of segments of the cultural stream. Archaeologists deal with individual cultures, but their goal should be seeking regularities that enable a clearer understanding of human behavior anywhere at any time (Haag 1959: 104).

Despite the near absence of evolutionism among Eastern archaeologists there has been a marked change in theoretical orientation over the last 25 years. This, we may believe, is almost directly attributable to the change in training of the active personnel. Whereas many leaders in Eastern archaeology at the beginning of the period were men from other disciplines who were attracted to archaeology almost by accident, most of the younger men entering the field today are trained in all phases of anthropology. Hence, they may bring to their archaeological problems the techniques and solutions applicable to ethnology, to learning theory, to sociology, and, perhaps, to linguistics.

If one examines the history of the development of any science or individual discipline, early in its story there is a necessary preoccupation with classification. This is often deplored because many students forget that classification is only a means to an end, that it is only sorting the cards in order to play the game properly. It is equally true that some students consider this operation an unconscious stacking of the deck, but no matter how it is done, it must be done. Kroeber once made the observation that the archaeological world of the early Americanist, Holmes, was essentially flat. In 1935, this "high key" concept of the archaeological past in North America was just emerging into a chiaroscuro whose shadows of the past have ever deepened since. Hence, more than 25 years ago, attempts were begun to render some order to the archaeological data that were then known. A group that met at the University of Chicago in 1932 for this purpose for the East included McKern, Cole, Griffin, Guthe, Deuel, and A. R. Kelly. The method that they appeared to have agreed upon became a guiding principle throughout the East. The eventual fate of this so-called Midwestern Taxonomic method is another excellent example of the destiny of a logical construct that proposes to meet virtually any conceivable situation.

When McKern (1939) finally published an article describing the system it was primarily to forestall further misuses of the method. Originally it was conceived to be used in an essentially objective sense with the more specific traits that distinguished sites and clusters of sites being derived from the generalized widespread items characteristic of the grander cultural expression called the basic culture or the pattern. The Midwestern or McKern method grew out of a dissatisfaction with the direct historical approach on the one hand, which could help archaeology to only a limited extent, and with ethnological and linguistic identifications on the other. The apparent promise that this method would bring some greater measure of order lead to its being widely interpreted as a genetic system more than a taxonomic one. That is, it seemed axiomatic that all manifestations having similar traits or a measure of similarity, were of common origin. It was not long before most Eastern archaeologists had expressed the content of their given areas in terms of the McKern classification. Although the original formulation of the system had expressly ruled out the factors of time and space, the later users of the system drew sweeping relationships on the basis of cultural similarity.

From the outset this system met with some criticism most amply ventilated in 1943 by Griffin (1943: 327–41) who demonstrated the more logically correct inductive application of the method, arguing for formulation of foci (groups of closely related components) from all traits of all components and deriving aspects and ultimately phases from these shared traits. What survives of the McKern classification system today is largely this method of application. In 1948, Guthe could write that this taxonomic method "was the single most constructive achievement in the eastern United States since 1925" (Guthe 1948: 9). From our vantage point now we can see that the McKern terms and the taxonomic method of comparison have taken on new meanings in new conceptual frames but that little else remains. It seems rather apparent that the gradual but fairly complete transfer of allegiance to other schemes is a normal growth pattern seen in most scientific disciplines, namely, first concern with descriptive classification, and then temporal matters.

Twenty years ago some of the first attempts at formulation of the culture history of the Eastern United States began to appear. These were possible mostly because of the detailed summaries for various subareas that were being produced, such as Ritchie's New York State account (Ritchie 1938). One of the most influential of these conjectural histories was that of Ford and Willey in 1941. This paper introduced a number of concepts that are still usefully employed (Ford and Willey 1941). The term "stage" for the major divisions of the culture history and their guess dates made this temporally-oriented account one that influenced chronological constructs of other students for some years after. Two decades after this appeared we may note with some surprise that the authors allowed only a thousand years for all Eastern prehistoric development. In 1946, the Johnson-edited volume, *Man in Northeastern North America*, contained another synthesis of Eastern culture history, this one by Griffin, who carefully refrained from being specific about time (Griffin 1946). Among other things, these articles marshalled our knowledge as of those times and enabled a closer look at needed field work. A joint project, involving Phillips, Ford, and Griffin, had begun in 1940 a survey of the Mississippi River Valley south of the Ohio River. When this volume appeared in 1951, it set the tone for several marginal studies (Phillips, Ford, and Griffin 1951). Not only did it resoundingly demonstrate the value of a knowledge of recent geology but it elevated the archaeological survey to a new height as an investigative procedure in prehistory.

A salient influence on the development of archaeology in the East was an increasing awareness of the importance of the contributions of other disciplines. The Phillips-Ford-Griffin survey was not the first to stress the importance of the physical setting in conditioning culture growth. The orientation in that volume was that their sequence of site relationships had a firmer foundation when it was based upon recent geologic history of so large a geomorphic feature as the Mississippi River Valley. But this study only tipped the scales from a heavy "history" direction to a weightier "prehistory" bent. Griffin (1959) recently said, "American archaeology may be said to have become a study of American prehistory when it discovered stratigraphy and seriation and when accurate techniques of determining prehistoric time were developed." Not all these desiderata came at one time, but there was a slow retreat from mass efforts with the shovel to the small controlled stratigraphic cut and the sorting table. Of course, the importance of geology to chronology was first realized in the West, but Eastern archaeologists were constantly alert to possibilities of the use of geologic association for dating. The Mississippi Valley situation gave ample proof of the usefulness of Recent geologic associations. More recently, McIntire's Louisiana coastal study admirably demonstrated how useful archaeology can be to coastal geologic history (McIntire 1958).

For some time archaeologists had been keenly aware of coastal fluctuations as a possible clue to antiquity. It was the Boylston Street Fishweir, however, that dramatically emphasized the great contributions to prehistory that were inherent in the geology and biology of an archaeological site (Johnson 1942, 1949). Not all sites lend themselves so fully to such large scale and broadly inclusive approaches as did the Fishweir. Nevertheless, the very fact that there was an awareness that it could be more fully used for casting light on Eastern prehistory was because of the then-changing thinking about problem analysis. A decade before the Fishweir excavation, Paul Sears had already indicated in an article or two that pollen analysis was a reliable indicator of past climatic fluctuations in Eastern United States (Sears 1932, 1937). Before 1940 dendrochronologists from the Southwest had invaded the Tennessee, Mississippi, and Ohio river valleys, half filling museums with paraffin-soaked logs (Hawley 1938, 1941; Bell 1952). The ghosts of these unsuccesssful time markers haunt the radiocarbon determinations today, yet dendrochronology in the humid East gave great promise of supplying highly localized time charts comparable to those in the semi-arid Southwest.

Additional botanical studies began in the mid-thirties by other Southwestern emigrants seemed harbingers of a better understanding of the ecological situations in which the Eastern past was nurtured. Jones' Kentucky rock shelter study was just such (Jones 1936). About this time, too, Mangelsdorf and Anderson began to unravel the obscure trail of maize, a tremendously vital subject to Eastern archaeologists, for maize was the basis of all higher culture (Mangelsdorf and Reeves 1939). Despite these brilliant beginnings, the last 25 years have not

seen a consistent growth in utilization of either geological or botanical data.

Carl Sauer, the most distinguished geographer of the last century, has had an indirect but apparent effect on Eastern archaeology. Rather than influencing archaeologists through theoretical constructs, Sauer has been felt most through the students he shared with Kroeber. An example is Fred Kniffen of Louisiana who attended the Birmingham Southern Prehistory Conference in 1932, was first to utilize the concepts of marsh subsidence and stream channel changes as method of arriving at chronology, and today chairs the first combined department of Geography and Anthropology in the United States. More directly influential is Julian Steward, but no less highly geographically oriented. Sauer's great forte has been the stimulating direction given his students, although it must be admitted that certain critics have used the words, "overstimulated" in some instances.

Not all this new direction of Eastern archaeology is local in origin, since certain Europeans materially affected the change toward ecology. Eiseley's exhaustive article on *Pollen Analysis and American Prehistory* written in 1939 rightfully recognized Paul Sears but most of his major citations are from the works of Northwestern Europeans (Eiseley 1939). British archaeologists were among the first of the world to change their outlook of the past from the historical approach to the ecologically-oriented prehistoric. The dynamic framework in which Childe, Clark, Daniel, Hawkes, and Piggott moved was based upon every available source of information and the quality of their published works led to their becoming textbook material for Americans. The exposure of Eastern archaeologists, notably Harvard graduate students, to European prehistorians eventually has affected students of the former. It is most probable that future Eastern regional studies will be increasingly directed toward an ecologically illuminated world.

If there is any single thing that typifies the Eastern archaeological endeavor of today, it would probably be the efforts at validating our analytic techniques. Statistical treatment no longer need cease with determinations of percentages of trait occurrences and subjective comparisons of trait lists. Spaulding has pointed the way — albeit, tortuous and forbidding to many who turned to prehistory to escape the mathematical reality of modern life (Spaulding 1953). Spaulding also has been a most vigorous defender of the concept of typology in archaeology which holds that types are revealed by "a process of discovery of combinations of attributes favored by the makers of the artifacts," and not by "an arbitrary procedure of the classifier" (Spaulding 1953: 305). Ford occupies the other polar position in this polemic holding that typology is a construct dependent upon the needs of the analyzer at given times (Ford 1954). Obviously no simple answer is to be suggested, but perhaps the soundest procedure has been that with which Rouse is identified — a combination of analytic and descriptive techniques that seeks process as well as history (Rouse 1960). The concern with classification today is greatly different from that of 25 years ago. Then, it was almost wholly descriptive, perhaps with identification of function the satisfactory goal, but now we look for genesis, for change in form and function through time, and for cause.

I would not agree with Guthe's selection of the Midwestern Taxonomic method as the most significant contribution in this quarter century. Rather, I believe that the introduction of the radiocarbon method of dating is far more significant than the development of the McKern classification method. The impact of this new device for ordering chronology has been marked by certain growing pains, of course, but it has supplied the shoring for our prehistoric frame of reference at a time when it was badly needed. Not the least important effect of radiocarbon dates has been psychological. The tranquilizing effect of an objective basis for a date is extremely reassuring. Phillips, Ford, and Griffin were aware of this when they labeled the chronology of the Mississippi Valley survey "our last opportunity for old-fashioned uncontrolled guessing." Later radiocarbon dates have indicated that they were still far too conservative.

In fact, the significant contribution of radiocarbon dating has been the revelation that Eastern archaeologists have consistently been too conservative. Radiocarbon has given us thousands of years more depth in time and, thus, thousands of years more time to develop the intricate patterning of the East. Also, these dates have given confidence to conclusions about culture lag: it *is* true that the Atlantic Coast Archaic survived to historic times, and it *does* seem that Adena was a retarded country

cousin of Ohio Hopewell rather than a dead grandparent. There still is a tendency to consider every date determined as a valid one for all manifestations of the culture type concerned, but this will be minimized as more dates become available.

With radiocarbon dating has come a renewed interest in Eastern culture history. Recently Caldwell's *Trend and Tradition in the Prehistory of the Eastern United States* (1958) and Willey and Phillips' *Method and Theory in American Archaeology* (1958) have appeared. Caldwell's study shows clearly the efforts being made to accomodate known archaeology in a now deepened time context but a no wider horizontal frame. It clearly shows how limited is our knowledge of the culture history of some parts of the East but how intensive is the documentation in restricted regions. It also demonstrates our immediate desire to emulate the European holistic approach without much of the necessary basic data. It is equally true, at least for the East, that the Willey and Phillips synthesis derives most of its life from ethnography. The basic argument is that tradition is the strongest influence in the culture history of given areas, hence, what persists in later stages are mostly elements socially inherited from earlier stages. The role of diffusion is not ignored, of course, but cultural continuity is emphasized at the expense of diffusion. Willey and Phillips were unable to extend their developmental systems "to continental or hemispheric dimensions."

No history can properly be ended on a note of the present. If we look to the future of Eastern archaeology a clearly defined horoscope may be seen. There seems little doubt but that archaeologists will not rush to the revitalized Boasian approach now called "conjunctive." It is quite apparent that questions are "being asked by anthropologists concerning common factors in the development of civilizations," but conjunctival contemplation of the interrelationships of archaeological components is not the fruitful procedure for the discernment of "cultural regularities." It seems much more evident that Eastern archaeologists will increasingly turn to the manifold resources of other disciplines. The utter impossibility of individual workers becoming proficient in these numerous fields suggests that we must follow the example of our British counterparts and seek to establish interdisciplinary centers, such as theirs for Quaternary Research. When this phase of archaeological activity is reached perhaps we may say that Eastern archaeology will have fulfilled its mission, and truly seeks to disclose the causative agents of changes in form and function through time.

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PLAINS ARCHAEOLOGY, 1935–60 *

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ABSTRACT

A review of archaeological research in the short-grass Plains and adjacent Prairie regions shows that although early travelers observed evidences of the prehistoric occupation of the area, no systematic archaeological work was undertaken until very recently. The publication of Strong's *Introduction to Nebraska Archeology* in 1935 opened the quarter-century of growth which was at first characterized by intensive surveys and testing, then by the excavations sponsored by the federal relief agencies, and since World War II by the large-scale salvage work connected with federal water-control programs, especially the Missouri Basin Project. These salvage operations have produced an enormous body of new data on all major time horizons and from all parts of the Plains, as well as an awareness of the rich potential of Plains prehistory. The scope of these operations has required a high degree of cooperation between research organizations and has led to the development of new excavation techniques, especially those involving heavy earth-moving machinery. Efforts to organize these new data have resulted in a uniform method of site designation and broadly applicable artifact classifications, but no scheme for culture classification to replace the Midwestern Taxonomic System which has been the conventional method of expressing cultural similarities. The direct historical approach to the problem of the ethnic identification of archaeological complexes, settlement pattern studies, and the relationship between man and environment, which have long been major interests of Plains archaeologists, continue to dominate the interpretive scene.

THE PLAINS AREA, whether as a geographic region or as a cultural one, has been defined in various ways by different workers. As an ethnographic culture area, it has been represented by Wissler (1931: 221) and Lowie (1954: 8) in their respective handbooks as extending from the mouth of the Missouri River westward to and beyond Great Salt Lake, southward to the Rio Grande, and northward to the Saskatchewan River. So delimited, it far transcends any "natural" region based on topography, native vegetation, soil types, or other non-cultural characteristics; and it includes several marginal tribes — for example, the Bannock on the west, the Iowa and Missouri on the east, who were at best only part time plainsmen.

In the present discussion, I have taken a much narrower view of the area to be considered. We shall be concerned primarily with the short-grass plains, or steppe, fronting on the Rockies — the Northern and Southern Plains areas of Kroeber — and the adjoining western portions of the Prairie areas to the east. In terms of historic native peoples, it thus includes the range of the "typical" Plains tribes of Wissler, that is, the mounted bison hunters, plus the less "typical" semi-sedentary Caddoan and Siouan Village tribes whose communities, like those of their ancestors or precursors, for some hundreds of years before the White contact period were strung along the major river valleys of the eastern, or Prairie, Plains. Although not all of this region belongs to the true Plains of the physiographer or plant geographer, practically all of it may be considered to have the major characteristics of what Webb (1931: 3-9) has termed the Plains environment. That is to say, the landscape is broadly characterized by flatness of land surface, by a grassland rather than a woodland flora, and by a semi-arid to sub-humid climate of continental nature.

From the earliest days of Euro-American penetration, this region has been a favorite one for the observation and study of its native inhabitants. The observations made from the 16th to the 19th centuries by explorers, traders, scientists, and others of European and American origin have assumed classic proportions in the anthropological literature; and the scientific publications of some of our largest museums and institutions of learning contain long lists of titles bearing on the native peoples whose colorful ways of life and stubborn defense of their traditional homelands and hunting grounds captured the imagination of all who came into contact with them. However, the relatively uniform character of these historic cultures obscured the diverse nature of their archaeological antecedents; most of the living tribes had traditions of former residence elsewhere; and the intellectual approach of the observers, usually conditioned by life in the humid woodlands of the eastern United States, was essentially that of Lewis H. Morgan when he asserted that the Indian shunned the prairies before he obtained the horse and the rifle. Thus, materials for the study of Plains prehistory did not begin to accumulate until much later; and soundly

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based interpretations are largely a development of the last 25 or 30 years.

I do not mean to imply, of course, that nothing worth noting in the study of Plains prehistory was accomplished before the 1930's, or that the present-day student can afford to be unaware of earlier activities. The journals and reports of many of the exploring expeditions, from Coronado through Lewis and Clark, Pike, Long, and others, contain nuggets of information that no scholar should ignore. Geologists, paleontologists, natural scientists, artists, government surveyors, missionaries, and Indian officials through much of the 19th century recorded relevant information, as did army officers surveying for reservation boundaries and military roads. Some of the earliest mound explorations of which we have record were by soldiers stationed at Ft. Atkinson, Ft. Leavenworth, Ft. Wadsworth, and in other frontier garrisons. It is to the non-archaeologists of the last century that we owe the first discoveries in the Plains of artifacts associated with extinct mammals, of a Pueblo ruin in the western Plains, and a little later, of Lansing Man, the Loess Man of Nebraska, and a host of other finds. More recently, the explorations of Will and Spinden on the Mandan, of Gilder and Sterns on earthlodge sites in eastern Nebraska, of Montgomery and Nickerson in the mounds of the Northeastern Periphery, and of various nonprofessionals widely scattered over the Plains, have given us additional data from localities to which few or no professionals had paid much attention even as recently as the 1920's. In the aggregate, these investigations brought to light a considerable amount of material of widely varying usefulness and reliability; but they produced no framework of prehistory nor any serious attempts at integration and broad scale interpretations. Thus, although a few observers in the region knew otherwise, it was still widely thought in 1934 that the Great Plains were "rather bare of archaeology" except perhaps in some of the larger river valleys; and even as late as 1948, the view was expressed that "the Plains as a habitat were only nibbled at, so to speak, by eastern farming tribes pushing into them intermittently, or by western groups better sheltered in the foothills of the Rockies" (Kroeber 1948: 823).

Late in 1929, the University of Nebraska Archeological Survey was organized; and in 1935, Strong's *Introduction to Nebraska Archeology* came off the press. Drawing principally upon fieldwork conducted from 1929 to 1932 and stressing the direct-historical approach, Strong pointed out, with documentation, the one-sided nature of the then prevalent concept of Plains culture. This work can fairly be said to mark the beginnings of systematic archaeology by professionals on a sustained and continuing basis in the Plains area. The most important developments in archaeology in its own right, instead of as a by-product of geology or paleontology or local history, have taken place in the past quarter century. It may be more than a coincidence that the decade of the 1930's witnessed the appearance of several other scholarly studies on the Plains as the habitat of man — including especially Walter Prescott Webb's classic study, *The Great Plains*, in 1931; two notable reviews of man and nature in the region by Clements (1938) and by Clements and Chaney (1937); and the report of the President's Committee on the future of the Great Plains (Great Plains Committee, 1936).

In the early 1930's, when Plains archaeology was cutting its teeth, so to speak, the methods and scope of its field operations had to be scaled to modest budgets and a skeleton personnel. Fieldwork was usually on an extremely limited scale, with volunteer help and a bare minimum of equipment. It consisted primarily of what was called intensive survey. In place of the extended and systematic sherd collecting from scores of sites, as came to be the practice in later years, these surveys were concerned more with limited tests or soundings — that is, with the opening of one or more house sites or cutting of a few test trenches in each of several widely scattered village or camp sites. Neither the time nor the manpower for large scale excavations were at hand; and the surface sherd surveys, it was felt, would not yield the wanted information on house types, village arrangements, and other aspects of what has since come to be known as settlement patterns. In other words, a good deal of information was being gathered about how these prehistoric plainsmen lived, and in what respects their lifeway differed from that of the historic groups, even though there was less information than was desirable regarding the general distribution of cord-roughened sherds or braced rims or corner-notched projectile points.

The opportunity for large-scale intensive archaeological operations in the Plains came with

the activation of the relief programs in the latter 1930's, specifically with WPA in 1937 and after. With this program came significant excavations in prehistoric village sites in Nebraska, on the Middle Missouri in the Dakotas, and in caves, bison kills, and other sites in Montana and Wyoming. Even earlier, FERA labor was utilized for site excavations in the Oklahoma Panhandle. Undoubtedly, more funds were channeled into archaeological work in the Plains during this decade than had been previously expended in the entire region in the 100 years since the impecunious but curious Rev. Isaac McCoy dug into a burial mound in northeastern Kansas in 1830 and briefly described its construction and contents. The enormous increment of specimens, maps, and other data from these large scale relief operations not only provided substantive materials with which archaeologists would busy themselves for years to come, but it also raised the serious need for classificatory and analytical methods and procedures to bring some order into the picture — geographically, chronologically, and with respect to the interrelationships of the various materials at hand.

With the close of World War II, Plains archaeology expanded as never before, in consequence of the activation of a country-wide federal water-control program and the prospect of hundreds of new dams and reservoirs to be widely scattered over the Great Plains and adjacent regions. The Inter-Agency Archeological Salvage Program that came into being in 1945 through the work of the Committee for Recovery of Archaeological Remains and as a direct result of this threat to our basic materials is now a well-known feature of the scientific landscape (Roberts 1952). A major unit of this program, the Missouri Basin Project, has been concerned with the salvage work in the central and northern Plains, operating in liaison with numerous state and local agencies, the latter in turn working with both federal and nonfederal funds (Cooper 1955; Annual Reports, Bureau of American Ethnology 1946 to present).

Prior to 1930, so far as the published record shows, no attempts appear to have been made to arrange Plains archaeological materials in any sort of systematic classification. Here and there, to be sure, a few amateur investigators had undertaken to apply distinguishing terms to their local findings, distinguishing their varying nature by such terms as Moundbuilders, Cremators, Modern Indians. Perhaps more often, thinking was in terms of antecedents of the historic peoples—that is to say, in terms of pre- or proto-Pawnee or Arikara or some other fairly well-known tribe, or else in terms of presumed relationships to other better known tribal or cultural entities in adjacent regions such as the eastern Woodlands or the Southwest.

There are, of course, various ways of ordering archaeological materials to show their degree of cultural similarity and their relative position chronologically. In the Plains, probably the first formulation set up by group discussion and enjoying a certain measure of agreement among several active workers from various states was that adopted by the First Plains Conference at Vermillion, South Dakota, in 1931 (Strong 1933: 276–7; Wedel 1936: 24–5). This was based on relative chronology of the broadest sort, and avoided any reference to tribal designations through the use of such terms as *Prehistoric* (that is, pre-White contact); *Protohistoric*, for materials showing evidence of White contact but not in specifically documented sites; and *Historic*, including those remains that were associated with specifically documented and identifiable sites. It was recognized from the outset that these categories were relative not only with respect to each other, but also from area to area—that the beginnings of White contact in one part of the Plains might well be a century or two earlier than in another. That the last two categories would include only a very small fraction of the number that would eventually be found to be prehistoric was also recognized. Nevertheless, the classification had utility and served a useful purpose particularly during the initial period of exploratory work in the region.

Relative chronology in the Plains has also been expressed in another three-fold scheme (Champe 1946: 87–92). Here, the periods are characterized by "that trait or complex of traits which seems most important to the archaeologist seeking a general classification." Thus, we have the *Lithic* period, beginning with the Early Hunting Peoples and running up to the appearance of pottery-making; the *Ceramic* period, extending up to about A.D. 1700; and the *Historic* period thereafter. In some respects, this seems a more satisfactory grouping than the preceding one, since the first two categories rest on criteria with which the archaeologist directly and regu-

larly concerns himself. Still to be determined, of course, is the extent to which the Lithic period in one section may have outlasted the beginning of the Ceramic period in another section. It should also be observed that, although the Ceramic period is generally terminated at A.D. 1700, both pottery and stone tools were still a very important segment of the material culture inventory of many of the Village Indians north of Kansas River as late as the end of the 18th century.

In 1944, in a notable contribution to the archaeology of the Middle Missouri, a fourfold classification was proposed for more than 100 village sites scattered along the mainstem from Grand River to the Yellowstone (Will and Hecker 1944: 53–69). Largely on the basis of ceramic typology, but with community patterns and architectural changes as important considerations, Will and Hecker grouped the sites into time periods with the following designations: Archaic Mandan, Middle Mandan, Later Heart River, and Decadent Ceramic. That these sites are all of Mandan origin is an assumption that might be challenged on methodological grounds, since it has not yet been demonstrated by controlled analytical methods that all of the Archaic sites, for example, can be linked through an unbroken series with the late documented sites of unquestioned tribal identity. If the proposed sequence can be validated when adequate samples of carefully controlled excavated data from all time levels are in hand, and the absence of any sharp breaks in the cultural continuity can be established, the culture history of this portion of the Middle Missouri will perhaps be seen as another example of the "branch" concept in Plains prehistory.

Still another classification is in use for the Northwestern Plains in Montana and Wyoming, where low-yield sites, simple material culture inventories, and scarcity of pottery has compelled workers to rely heavily on changing projectile point types in establishing period differentiations. Four periods are recognized: an Early Prehistoric, a Middle Prehistoric, a Late Prehistoric, and a Historic (Mulloy 1958: 140–223). The first three of these are again subdivided, each into an Early and a Late subperiod or phase, or, in the most recent unpublished version, into an Early, Middle, and Late stage. It is not always easy for the outsider to keep his bearings here when comparisons are being made between, let us say, the Late Early Prehistoric and the Early Middle Prehistoric, or to escape a certain bewilderment when comparisons are made between the Late Early Prehistoric and the Early Late Prehistoric periods, even when there are perfectly valid grounds for such divisions from the cultural standpoint.

Except in the Northwestern Plains, the conventional method of expressing cultural relationships in the Plains area is the Midwestern Taxonomic System. It was first applied in two papers published in 1935 and presenting the results of several seasons of survey and excavation by the University of Nebraska Archeological Survey and the Nebraska State Historical Society Archeological Survey (Strong 1935: 1–2; Wedel 1935: 251–5). Expressed or implied, and with frequent changes in terminology (for example, Stephenson 1954), this system has formed the basis for most classificatory operations since. From North Dakota to Texas, especially where the ceramic cultures are involved, the unit terms of component, focus, aspect, and phase are customarily used in allocating archaeological complexes and assemblages, and for grouping them in categories at various levels of synthesis. For the larger categories, such as phases, some of the terms used are directly equated with those previously named in the Midwest, such as, Hopewellian, Middle Mississippi; but there is not yet general agreement on either the naming or the validity of other named taxonomic units in the scheme as generally applied in the Plains region. It seems safe to predict that this system will continue to be used for some time in the future.

The most recent classificatory device introduced into the Plains picture is the concept of the "branch." As proposed for the Middle Missouri in central South Dakota in the Fort Pierre Branch (Lehmer 1952, 1954), this emphasizes cultural continuities rather than the fragmentation or compartmentalization of the archaeological data that usually results when aspect lines are drawn between the various foci found in a particular district. Thus, since certain features of potterymaking, house construction, and other items in the material culture inventory continue from one cultural unit, or focus, into a later one which on general grounds might be classified into another focus or aspect, it has been argued that the concept of the branch more accurately reflects local culture history than does the procedure usually followed by the classifier using the Midwestern system.

The concept has not yet been applied with equally well-controlled masses of data to other sections of the Plains, and its wider utility thus has not been empirically demonstrated. It is undoubtedly true, too, that when we have better insights and more systematized data, Plains prehistory will be clearly seen to have a pattern that resembles less the dendritic structure suggested by the term "branch," and more as a complex network, in other words, as more or less reticulate.

The river basin salvage program which has been under way since 1946 has made of the Plains area a testing ground for large scale archaeological studies. Unlike the relief projects of the 1930's, the salvage operations have not been under the necessity of concentrating primarily in localities where there was heavy unemployment, regardless of the archaeological possibilities of those localities. Instead, charged with the responsibility for locating and evaluating any and all sites that would be flooded by reservoir construction, salvage archaeologists were able to make spot-checks, and then more thorough surveys, of innumerable localities which very often would not normally have been investigated for many years. Geographically, the sampling thus made has included every section of the greater Plains area here under consideration—the Northwestern Plains in Montana and Wyoming, and northward into the Canadian Prairie provinces; the Middle Missouri in the Dakotas and the Northeastern Periphery east and northeast of the Coteau du Missouri; the Central Plains of Nebraska and Kansas; the High Plains of eastern Colorado, western Oklahoma, and eastern New Mexico; and the Southern Plains of Oklahoma and Texas. A wide variety of local environments is also represented—rich alluvial valleys along the Missouri and the lower reaches of most of its major tributaries, where aboriginal horticulture formed the basis for settled life in sizeable communities; lesser stream valleys throughout the loess plains, in the short-grass country, and around the Black Hills, in zones transitional between the horticultural eastern plains and the primarily hunting country of the western plains; semi-arid sagebrush plains in Wyoming, always predominantly a hunting and gathering area; and mountain parks and high lake valleys up to 10,000 feet elevation in Montana and Wyoming. Field investigations have run the gamut from Early Big Game Hunter sites of seven to ten thousand years ago, through the later lithic and ceramic horizons, to sites of the historic fur-trade and western-expansion period. Out of these surveys and the sustained excavations that followed has come a great mass of new data, along with a greater awareness of the richness and abundance of prehistoric remains in the Plains, western as well as eastern.

The salvage program, aside from the vast collections and other substantive data amassed, has had wide ramifications methodologically and in other respects. It has made possible a widespread systematic attack on the archaeology of virtually the entire Plains region, with preliminary surveys or spot-checks, intensive follow-up surveys with soundings designed to give a firmer basis for site evaluations, careful selection of key sites from among the many available, and finally large scale excavation and laboratory analysis. It has led, I think, to closer coordination of efforts by all concerned, federal and nonfederal agencies alike, than was previously possible. It has permitted the thorough testing, on shallow village sites as well as on deeply buried ancient campsites, of heavy power machinery, thereby revealing village plans and other features of native community life that were not ordinarily recoverable by hand operations, where the manpower and funds available are limited. From this experience, it can be suggested, even insisted, that the bulldozer, ditch digger, and road patrol, properly and judiciously employed, can be quite as useful a part of the archaeologists' tool kit as is the shovel or the whisk broom (Wedel 1951).

Not the least of the benefits from salvage archaeology is the fact that a direct outcome of the greatly accelerated and widened activities has been the development of a uniform system of site designation applicable throughout the entire Plains region and indeed beyond. In key areas such as the Middle Missouri and in regions of low-yield camp sites such as the Wyoming Basin, the salvage program has made possible work on a continuing year-to-year basis, following up specific problems of culture growth whose eventual correlation will greatly increase our understanding of culture history in the region at large. In the laboratory, ceramic classifications for the eastern Plains and projectile point classifications for the largely potteryless Northwestern Plains, have developed or improved in response to the need for handling ever-larger bodies of artifacts and supporting data. Finally, the or-

ganized chronology program set in motion in recent years, utilizing dendrochronology, radiocarbon dating, geological-climatic data, pollen analysis, proportional counter, and other methods, promises to give us something more substantial than guess-dates to hold together the increasingly complex structure of Plains prehistory.

From the first, and especially in the eastern Plains or Village Indian area, one of the principal considerations in archaeological research has been the problem of linking the historic tribal groups with the older remains that litter the area. Thus, the direct historical approach has long been a favored avenue of attack, though sometimes implicit rather than expressed. The "typical" tribes of historic times were traditionally all late comers to the region, and the archaeological evidence from the Northwestern Plains and southward through the High Plains seems to be, in the main, confirmatory of this view. It would be unwise and incorrect, however, to conclude that these late arrivals moved into a previously uninhabited region, since the archaeological record furnishes abundant proof of earlier peoples here. As for the Village Indians of the eastern Plains, there is incontrovertible proof of long-continued occupation, with the record gradually lengthening as more archaeological data and better chronological controls come to hand.

When the Nebraska Archeological Survey was organized in 1929, a notable beginning toward defining the archaeological antecedents of the Pawnee had already been made by A. T. Hill and other non-professionals; and work since that date and preceding World War II has made this group and its history perhaps the best-known in the Plains region. Farther south, the way has also been pointed toward tracing out the antecedents of the Wichita and closely related groups of the Southern Caddoan stock. On the Middle Missouri, with the extensive digging of the River Basin Surveys and various state agencies, there is the very strong likelihood that when fieldwork has been reported in full, the prehistory of the Arikara will be as well known as that of their southern Caddoan-speaking kin. The problem of the Mandan has been coming into sharper focus since the pioneer work of Will and Spinden, and their origin now seems well on the way to solution. The Siouan-speaking groups along the Missouri River in eastern Nebraska and downstream have also come under increasingly close scrutiny; but, except for the Missouri and Osage on the lower river, there is still little substantive matter in print from which one can judge the results of these studies.

In the southwestern Plains, the intriguing problem of the Plains Apache has been reduced to manageable proportions, and their identification with the Dismal River culture gives the archaeologist and the ethnohistorian a common meeting ground for further inquiry into the earlier history of the group (Champe 1949; Secoy 1951; Gunnerson 1956; Wedel 1959: 589–99). In light of the ethnohistorical and archaeological correlations now possible, there seems to be no valid reason for questioning, as Lowie did as late as 1954, the existence of a large Apache population in the southern Plains at least as early as Coronado's time. That the Jicarilla Apache in 1934 "insisted on the ancillary and comparatively recent use of the tipi," and "psychologically are anything but a Plains people," are interesting items (Lowie 1954: 7, 192); but as evidence that the Apache of the 16th, 17th, and early 18th centuries were not plainsmen, they inspire far less conviction than do the contemporary Spanish documents.

The relationships between the natural environment and the distribution and ways of life of man in the Plains have also come under increasingly close scrutiny during the past 25 years (Wedel 1941, 1953). As Webb showed in his well-known study, *The Great Plains*, the men who left the wooded and well-watered lands of the eastern United States for the open grasslands were faced with the need for major cultural readjustments in order to survive. Archaeology has been able to contribute its own data and to add perspective to the story by correlating cultural sequences with buried soil horizons, and native horticultural distributions with climatic patterns. It is perhaps still too early for us to say positively that droughts in prehistoric times were critical factors in the movements of native populations into, out of, or through the Plains, though it seems highly probable that the native groups must have been adversely affected at times by unfavorable climatic conditions, just as were their white successors in the region. What is clear, at any rate, is that the Plains environment has not imposed such severe limitations on aboriginal activity in the Plains as was commonly supposed before archaeological studies got under way; and alternatively, that to

the extent that prehistoric groups attempted corn-growing in the western and northern Plains, the strong probability exists that the uncertain and irregular nature of the rainfall, of unseasonable frosts, and of other climatic factors may have been as important in determining the needs and directions of human re-adjustment as they were for the white man and the farming Indians of the eastern Plains in the historic period.

As one aspect of the problem of human ecology in the Plains, the manner in which native man disposed himself over the landscape has been a subject of continuing interest since at least the 1930's (Wedel 1956). In the Central Plains, even before systematic and organized research got under way, limited digging and surface surveys by non-professionals had provided clear leads to the marked differences between the prehistoric farming communities some hundreds of miles west of the Missouri and the villages of historic times in the eastern Plains. The settlement pattern concept, or something very like it, was already in the thinking of some Plains workers by 1935. Intensified researches since have made much clearer the contrasts between the small, unfortified, loosely arranged rural hamlets of the 13th and 14th centuries and the large compact, often fortified, towns of later days in the eastern Plains. Today, we know further that a roughly similar change in community patterns took place in other parts of the eastern Plains, but with important differences in detail, and that other peoples throughout the region had widely varying community and house types, and sharply differing lifeways. As is probably to be expected, the semi-sedentary communities of the food-producers of the eastern Plains were usually characterized by relatively permanent habitations, whereas the earlier food-gatherers who persisted into late times in the Northwestern Plains made out with far simpler and more rudimentary dwellings and smaller communities. There is still much we are not sure about in connection with settlement patterns, as for example, the actual numbers of inhabitants residing in the prehistoric communities, and the density of population in various sections at various time levels; but with the accumulating data of the past few years and the insights we now have, I believe the future holds much promise for further fruitful inquiries.

In the Plains, as elsewhere, publication of results has lagged far behind the fieldwork; and so synthesis, based on considered statements committed by workers to the printed page in contrast to "preliminary" oral discussions, has also lagged. It is abundantly clear, nevertheless, that the past 25 or 30 years of archaeological endeavor have drastically altered the older concepts of human prehistory in the region. No longer need we wonder with Wissler (1907, 1908) about the "prehistoric uninhabited region in the western part of the Missouri-Saskatchewan area," or accept his suggestion that "the peopling of the Plains proper was a recent phenomenon due in part to the introduction of the horse and the displacement of tribes by White settlement," or the further view that the "tribes of the buffalo country never rose above the cultural level of nomadic hunters." Nor can we accept Kroeber's (1939: 88) argument for the possible lateness of the Folsom complex or his more recent (1948: 823) observation that "the conical skin tent dragged on poles" and the "bedding, clothing, and even receptacles of skins and rawhide" of the horse Indian days "had no counterpart in the region even as late as 1600," which is directly contradicted by the Coronado documents of 1541.

In the light of archaeology, it is now generally recognized that the peopling of the Plains was underway at least as early as ten or twelve thousand years ago, when the Early Big Game Hunters pursued elephants and bison in the lush grasslands of the western plains. There are those who regard this estimate as ultra conservative, as indeed it may be. Later, and perhaps in some way linked with the climatic adversities of the Altithermal period, hunters and gatherers, or foragers if you will, overspread the Wyoming Basin from the deserts of the Great Basin, possibly moving on into much of the Plains toward the northeast. Other Early Hunters with a notched-point tradition appear to have been hunting *Bison occidentalis* in the eastern Plains about as early as the "Yuma" hunters in the west or quite possibly even before. At least partly because of environmental limitations, the western and Northwestern Plains have always been primarily a region of food-collecting subsistence economies, that is, of hunting, or of hunting and gathering, lifeways, in which cultural development and population were held to a relatively low level until the introduction of the horse about two centuries or more ago. Even in this section, however, most of the earlier pre-archaeological views of prehistory are only partly

correct, or else apply only at a relatively late time level.

In the eastern Plains, on the other hand, environmental conditions permitted the addition, perhaps two millennia ago or earlier, of a food-producing economy to the previously existing hunting and gathering lifeway that presumably was rooted in the eastern Archaic. On the more assured subsistence basis provided by maize and other domesticates, there developed here a succession of semi-sedentary, pottery-making, village-dwelling cultures, characterized by steadily increasing population aggregates, drawing many of their practices from the more advanced peoples to the east, and sharing the Plains in the final century or two of their existence with the mounted bison hunters.

Summarizing briefly, in the past 25 years we have seen Plains archaeology grow from a modestly supported pastime engaged in chiefly by non-professionals to a serious business with a score or more professional practitioners and substantial financial resources. Vast new bodies of carefully-gathered field data have come to hand, and specific problems have been, and continue to be, recognized in the programming of future work. Culture chronologies and sequences have been promulgated for nearly all sections; and, despite their often tentative nature, the general framework of Plains prehistory is becoming measurably clearer. Many gaps still remain, to be sure; but in approaching them, we are able to stand on an increasingly solid foundation of what we believe are established facts or sound premises. In the years that lie ahead, we may hope that comprehensive and thoroughgoing studies of the available skeletal materials by physical anthropologists, distribution studies of specific traits at different time levels on a regional basis, and full analyses of the masses of unpublished data now at hand in most of the active research institutions of the region, will give us progressively clearer insights into the story of human settlement and culture growth in the Plains region.

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THE GROWTH OF ARCHAEOLOGY IN THE WEST COAST AND THE GREAT BASIN, 1935–60 *

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ABSTRACT

Archaeological research in the Far West of North America is reviewed in terms of the contributions to methodology and theory. The generally simple level of culture in an area in which functioning native cultures survived until recent times has led to the perfection of analytical techniques for non-ceramic evidence, the development of ingenuity in deriving conclusions, and a considerable use of the direct-historical approach. New methodological approaches have been developed in analysis of physical components of sites, determination of food resources, estimation of population density, establishment of functional typologies, testing of the value of chemical and other changes for chronological control, and experimentation with statistical techniques of seriation. Theoretical trends include a lack of culture classification systems comparable to those in other North American areas, a dependence on classificatory units on the level of the complex or assemblage, and a tendency toward the development of a classificatory system based on an ecological framework.

THE WHOLE business of archaeology consists of getting the most information out of fragmentary data. Workers in the West share a common effort with other archaeologists in attempting to derive the maximum knowledge of past peoples from the material remains found in archaeological sites. As in other regions, there has been a tremendous growth of archaeological knowledge in the Far West. Yet in reviewing the progress of western archaeology, it would be impossible to summarize hundreds of individual excavation projects with all their contributions to new understanding of chronology, ancient man, and culture growth in general. Hence, while the primary importance of data-gathering in the form of site reports, distribution studies, and the like, is not neglected in this review it seems most appropriate to be less concerned with *what* was found, and more concerned with *how* it was found. The examples I have chosen to cite, therefore, have some quality of novelty in that they bring new ideas, or new applications of old ideas, to the perpetual task of archaeology in gaining the fullest picture from the clues of the past. To amplify further, the two kinds of progress in archaeological scholarship are:

1. New methodology — new techniques for deriving more information from archaeological evidence, such as a new way to discover chronological relationships. The methodology itself may also be of two kinds: field methodology (excavation techniques) and analytical methodology (new ways of interpreting collections).

2. New applications of known methodology, in which a system developed previously is applied to a new area or problem. There is, of course, considerable overlap between the categories, and I have not tried to grade the degree of originality in the examples to follow. The examples are chosen to show the areas of interest to western archaeologists, the growth of their thinking, and the results that might be useful to workers in other areas.

The nature of a region's archaeology has a large effect on the kind of methods and analysis appropriate to scholarly study. The entire West Coast and Great Basin can be categorized as having rather simple levels of aboriginal technological and social development, even in comparison with much of the rest of the United States. Archaeological cultures in the Far West tend to be *relatively* meager, simple, and slow-changing throughout, yet there are infinite variations on a basic hunter-gatherer theme. For this region it is seldom feasible to be concerned with many of the studies having value in richer and more complex areas (style analysis, for example). On the negative side, this situation sometimes makes the archaeologist feel that he is spending a mountain of effort to attain a molehill of interpretation.

On the positive side, archaeologists of the Far West have often been able to see essential cultural factors without drowning in a sea of potsherds. Further, by the very nature of their material they have been stimulated to the maximum ingenuity in deriving conclusions. Finally, this is a region which had functioning native cultures until very recent times, and accordingly the archaeologist has an unusually rich ethnographic base on which to build. The whole of the Far West is an area in which the "direct-

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historical approach" is feasible and useful, and nearly all western archaeologists have made some use of this approach.

From the foregoing, it is evident that Far Western scholars would move early in the direction of careful measurement, quantification, and experimentation with all kinds of correlations (artifact weight to function, site area to population, number of shells to dietary protein, and many others mentioned below). Not all of these attempts have been unqualified successes, yet they typify the intellectual history of western archaeology.

The areas of research in the West that have seen the development of new approaches may be summarized briefly as follows:

1. Analyses of the physical components of archaeological sites (Cook and Treganza 1947; Treganza and Cook 1948; Cook and Heizer 1951). A large variety of aims and results has been attained here, since midden components have been analyzed to yield data on such topics as: (a) quantitative data on food resources and nutrition (Greengo 1951; Cook 1946, 1950; Meighan 1959b; and several others—for further bibliography see references cited); and (b) population density (Cook and Treganza 1950; R. Ascher 1959). Such analyses are made in many regions, but the western workers have often gone beyond mere qualitative study (for example, identification of animal species present) to measure the relative proportions of different food resources. Through this interest, western archaeologists have developed a number of refinements in methodology, particularly with respect to sampling methods and the attainment of quantified, rather than impressionistic, interpretations. The approach is quite similar to that used by Clark in the Old World (Clark 1952; 1956: 10).

2. Krieger's concept of analysis by use of whole vessels rather than fragments, developed with pottery at the Davis site, was continued with basketry containers at Humboldt Cave, Nevada (Heizer and Krieger 1956). This is an attempt to deal with the basketry complex as a cultural entity, rather than simply an enumeration of basketry fragments.

3. The notion of equating population with "fish-miles" (miles of fishing stream) developed by Baumhoff (1958) refines previous concepts of geographical determinants to yield estimates of prehistoric populations. Also in this category can be mentioned the correlation formula of Cook and Treganza (1950) showing relationship between population and site area. These, plus Ascher's technique of midden analysis (R. Ascher 1959) provide a number of methods for estimating, with reasonable accuracy, the population of prehistoric hunter-gatherer groups. Since studies involving geography, ecology, or cultural complexity must all take population density into consideration, it is of extreme importance to interpretation that more adequate estimates of prehistoric population size be developed. The works cited are among the few systematic attempts in this direction.

4. Cressman (1956), in the Kawumkan Springs report, attempts to make explicit recognition of the differing amounts of excavation at different levels by use of an "adjusted" typology, equalizing the collection as if a standard amount had been dug from each level.

5. Fenenga (1953) shows a correlation between the weight of projectile points and the weapon used (bow and arrow or spearthrower). Although it has been shown that this correlation is not a functional necessity of the weapon employed, there is increasing evidence to support it as a true historical relationship. Demonstration of such correlations is of great value to interpretation since they permit the inference of a perishable artifact (in this case, the weapon) from some attribute of a non-perishable artifact (the weight of the point).

The always-important questions of chronology have been handled in the West by the conventional methods used elsewhere, as well as by research into new chronological methods. Dendrochronology, so important in the adjacent Southwest, has not been developed in the Basin and Pacific Coast regions, although several studies, most recently those of Ferguson (Ferguson and Humphrey 1959) indicate the feasibility of dendrochronology in the Far West if sufficient wood can be obtained from archaeological contexts. Contributions to new chronological methods include:

1. Thermoluminescence (Kennedy), although still in the experimental stage, holds promise of providing a new dating tool of particular value for sites yielding pottery.

2. Many possible factors in chronology have been studied through analysis of chemical changes in the fossilization of bone (Cook and Heizer 1952, 1953, and several additional pa-

pers). The results are again inconclusive but the work may yield more precise chronological indicators in the future.

3. Some attention to changes in soil through time has been paid by Carter (1956) and others (Harradine 1953; Storie and Harradine 1950). This is a most promising field for investigation, still in its infancy so far as western archaeology is concerned.

4. Another experimental dating method, involving workers from several areas, is that dealing with hydration rates of obsidian. In the West, such studies are being done by Donovan Clark, who has obtained encouraging results from west coast sites.

5. An interesting possibility for dating, although of limited application, is the observation of annual deposits of travertine in certain cave locations. The possibility of using travertine deposition as an indicator of age has been clearly shown for Moaning Cave, California (Broecker, Olson, and Orr 1960).

In addition to these diverse studies, several western scholars have been actively concerned with internal dating problems and site comparisons based on seriation. Although the notion of seriation is an old one, considerable experimenting with seriational techniques has been published (Brainerd 1951; Robinson 1951; Dixon 1956; Belous 1953; Bennyhoff 1951; Meighan 1959c; M. Ascher 1959). A particularly interesting application of statistics to support a seriation of petroglyph styles has recently been published (Baumhoff, Heizer, and Elsasser 1958).

Except for the business of taking small site samples for midden analysis, and one brief paper evaluating shovel archaeology (Meighan 1950), there has been little western development so far as excavation methodology is concerned. The past few years have seen a marked increase in the use of power equipment in excavation, shared with the rest of the country and stimulated in part by the necessities of "rush" jobs of salvage archaeology (Gilman 1961). However, excavation methodology would seem to be an area in which further refinement is both feasible and desirable. I have a feeling that in the West, once a site is selected the actual excavation is still too much on a "hit or miss" basis of digging random pits and trenches with no real conception of the best way to go about the digging process. Since it is rare indeed for more than a small part of a site to be excavated, the selection and excavation of that small part is too important to be left to chance. Unfortunately, we still have no developed methodology that will permit us to excavate, say a 10% sample of a midden, with any assurance that we are getting the most informative 10% possible. Such problems represent a most important area for research in those regions where hunter-gatherer sites predominate. A preliminary attempt in this direction, combining a number of excavation techniques on one site, has been presented by McKusick (1961).

In this brief survey of the methodological aspects of western developments, it is interesting to note that the scholars cited are often not professional archaeologists, but have their primary affiliation with another discipline. Those cited thus far include professionals in the fields of physics, geophysics, soil science, geography, geology, physiology, mathematics, architecture, and sociology. This typifies the way in which archaeologists make use of other disciplines, not only for data (as in the identification of animal bones, for example) but also for methodology.

From questions of methodology, I turn to the more difficult matter of archaeological theory, as it applies to cultural developments in general. Here, there is a virtual absence of contributions from other disciplines, and the development of theoretical formulations has quite rightly been left to the archaeologist himself. However, our satisfaction in this may well be tempered by the fact that American archaeology in general has a disconcertingly small body of theory about culture change. Although implicit and unpublished theory plays a large part in archaeological work, only a few rather tentative formulations of general scope have been published by archaeologists of the Far West. Such archaeologists, like those elsewhere, can plead lack of substantive data as the reason for their limited attention to theory. It is probably true that theoretical formulations are just now beginning to be possible for the Far West, and it can be expected that the next few years will see much more of this kind of writing from the Pacific Coast and Great Basin. Whatever the reason, there is little in the way of published theory, and I will here simply summarize one aspect of what I see as implicit theory apparently shared by most western workers.

I think it is interesting that Far Western archaeologists have in general shied away from the kinds of cultural classification systems developed in other parts of the country. The Pacific and Great Basin regions (with some exceptions) have not worked with phases, stages, standardized chronological periods, and the like. This situation no doubt began because of spotty information (both areally and temporally), but at the present time I would suggest that the continuing "reluctance to classify" characteristic of the West is based on a general feeling that we need more meaningful kinds of classification, and on a desire to avoid a rigid system that might predetermine the interpretation of culture history. The result is that most archaeologists of this region operate conceptually with a "complex," something most comparable to the Old World "assemblage." Since there are some hundred or more of these named units, the impression given the outsider is one of rampant individualism, with each worker coining cultural names with little reference to what anyone else is doing. However, we can hope that out of this loosely structured system will develop some new ideas about cultural classification and cultural history. There have already appeared a number of regional syntheses in which the historical development of a particular area is seen as structured by one or more defined causal factors, such as geography or ecology. These cannot be considered studies in theory but are worth mentioning as efforts in the direction of theory. For the Great Basin can be mentioned Cressman (1956), Jennings and Norbeck (1955), and Jennings (1957). For California, there are Heizer (1958), and Meighan (1959a). For the Northwest Coast, an even greater abundance of this kind of writing is available, including Borden (1954), Smith (1956), Osborne, Caldwell, and Crabtree (1956), and several others. Most noteworthy is the recency and close temporal grouping of all these papers, giving some reason for optimism about the development of archaeological theory in the West.

There is currently a tendency throughout the Far West toward development of a classification system based on an ecological framework. Although this does not sound particularly novel, it must be pointed out that there is a difference between a classification system *based* on ecology and one that simply includes ecology. For instance, the so-called paleo-Indian cultures are still recognized primarily on the basis of artifact types, and the fact that most of these people were big-game hunters is only incidental to the classification scheme itself. And further, even if "big-game hunters" is used as the starting point for the classification, it is necessary to recognize different kinds of big-game hunters before the system can have anything beyond a very general interpretive value.

I believe that several workers in the Far West have been feeling their way toward a classification system based on adaptation to a particular ecological niche. Much of the California work (including the midden analysis projects), Cressman's recent work in Oregon, and large parts of Great Basin archaeology, can be seen as bearing in this direction. This approach is still in its exploratory stages, largely because of the difficulty of recognizing and defining the fine "ecological categories" necessary to such an approach. Perhaps the most successful broad attempt in this direction has been the work of Jennings and his co-workers in recognizing and defining the Desert Culture. This conception of a cultural continuum, structured throughout by the restrictions of a harsh desert environment, places a large number of simple and not very exciting artifact assemblages into a meaningful culture-historical context. Refinements of the broad picture can best be made by looking at differences in Great Basin ecology rather than differences in arrow points or other artifacts. Or, to put it another way, it is the ecological niche that gives real meaning to the artifact types.

It is impossible, in a paper of such short scope, to mention even briefly all the ways in which workers in western archaeology have borrowed and utilized ideas developed elsewhere. It must be recognized, however, that many scholars besides those mentioned here have had a hand in strengthening our knowledge and in contributing to the theoretical, as well as the substantive, aspects of Pacific Coast and Great Basin archaeology.

This point is best shown by mentioning the sheer volume of knowledge that has been assembled in the past 25 years. Volume 1 of American Antiquity had an article on California archaeology, and all but one of the succeeding volumes has had at least one article on California work. To some this seems a disproportionate amount of space for an area not noted for rich archaeology, but it is simply a reflection

of the greater number of archaeologists in California, which has more members of the Society for American Archaeology than any other state. Despite this relative density of archaeologists, a long history of interest in archaeology, and a climate permitting field work all year around, California archaeology in 1935 was almost unknown. Only six of some 75 named cultural complexes had been recognized in 1935, and in 1936 Kroeber could publish a rather pessimistic article, "Prospects in California Prehistory," in which little beyond a skeletal outline of chronology could be presented. A somewhat similar growth of knowledge can also be shown for the Great Basin, Oregon, and Washington. It seems fair to say that over 90% of our knowledge has come since 1935, and that the development of Pacific Coast and Great Basin archaeology has been closely co-terminous with the growth and development of the Society for American Archaeology.

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TWENTY-FIVE YEARS OF ARCHAEOLOGY IN THE GREATER SOUTHWEST *

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ABSTRACT

Archaeological research in the Southwestern United States is reviewed in terms of advances in interpretation and in method and theory during the past quarter-century. Interpretative advances include the clearer understanding of the early Big Game Hunters, the filling of the gap between the early hunters and the sedentary villagers with manifestations of gatherers and collectors such as the Cochise and Desert cultures, the demonstration that the Desert culture has an antiquity comparable to that of the Big Game Hunters, the definition of the Anasazi, Hohokam, Mogollon, Sinagua, and Patayan variants of the sedentary, pottery-making agriculturalists, and the delineation of the connections between the Southwest and the northern Mexican portion of Nuclear America. Advances in method and theory have been most evident in the control over chronological problems, the development of interdisciplinary approaches, the establishment of culture classifications and pottery taxonomy, and the attempt to achieve problem-oriented archaeology in large salvage projects. The Southwest has long been characterized by long-term excavation at single sites or in small areas and by local development of academic and field training programs in archaeology.

TO ATTEMPT to synthesize 25 years of archaeological activities in such a vast area as the Southwest, and one in which an array of diggers have at times almost been throwing spoil dirt into each other's trenches, has led me to include in the following statements numerous generalities, some of which I realize suffer from over-simplification. Statements on the Southwest such as this have been almost as regular as Hopi Winter Solstice ceremonies; however, I have tried to make this paper a little different by soliciting opinions from a number of my colleagues active in Southwestern archaeological research, and by incorporating some of their ideas and opinions in this presentation. I greatly appreciate their response to my inquiries, but I will not implicate them in the shortcomings of this paper by allowing them to remain anonymous.

The area to be considered includes most of the states of Arizona, New Mexico, Colorado, and Utah plus portions of adjacent states to east and west, as well as sections of Sonora and Chihuahua, Mexico. I will not take time to describe this region, as I am sure readers are familiar with its deserts, plateaus, and mountains and the fact that its aridity, lack of ground cover, and presence of natural sheltered habitation sites have influenced the developments of Southwestern archaeology.

Considering the last 25 years places the beginning of this review at 1935. At that time I was an undergraduate student at the University of New Mexico and well remember being shown an announcement of a new journal, AMERICAN ANTIQUITY, by one of my professors who recommended that I should invest $3.00 in a subscription. The first issue contained an article entitled "Field Work in North America during 1934." It consisted of two parts and was continued in Number 2 of Volume 1. Reviewing the brief reports in those articles fairly well reflects the interests of Southwestern archaeologists at that time and furnishes us with previews of several important accomplishments and trends that were emerging in Southwestern archaeology.

Among the field activities listed in those first issues of AMERICAN ANTIQUITY were the following:

Arizona. Harold S. Gladwin wrote that the staff of Gila Pueblo began excavation of a ruin near Snaketown and that work was still in progress. This of course resulted in the comprehensive reports on Snaketown and on the Hohokam in general (Gladwin 1937, 1942, 1948; Gladwin, Haury, Sayles, and Gladwin 1937). Gila Basin archaeology was coming of age. The Gila Pueblo Conference had been held in 1931 and a sequence of culture stages for the Hohokam had been set up.

Ansel Hall reported on the Rainbow Bridge–Monument Valley Expedition in the Kayenta area which was particularly interested in investigating the transition from Pueblo II to Pueblo III culture stages. This program indicated concern with better defining Basketmaker-Pueblo culture stages and relationships and in filling temporal and areal gaps, an activity in which numerous Southwestern archaeologists were involved.

Byron Cummings wrote of investigations by Arizona State Museum in Pueblo remains of

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western Chihuahua and cave pueblos of the Sierra Madre Occidental. Interests in northern Mexico were developing.

Harold S. Colton described excavations at Wupatki and other sites in the vicinity of Flagstaff. The Sinagua and Patayan were evolving.

Colorado. Frank H. H. Roberts reported excavating part of an important ancient camp site and workshop near Fort Collins. He described the presence of a Folsom assemblage associated with bones of extinct bison. Also the presence of fireplaces and pits evidenced the first Folsom camp site (the Lindenmeier site) thus far discovered. Studies of Early Man were being placed on a solid basis after the initial impact of the Folsom discovery.

Earl Morris wrote of repair and restoration at Mesa Verde National Park and Aztec National Monument. Concern of the National Park Service with the excavation and stabilization of archaeological sites was reflected by these activities. Also reported by Morris was a search for datable timbers from Basketmaker III and Pueblo I sites to be submitted to A. E. Douglass. Tree-ring chronology had proved its worth and the master chart was being verified and expanded.

Paul Martin described excavations at the Lowry Ruin. Again, interest in the distribution of cultures and subcultures was reflected by this excavation program.

New Mexico. Harold S. Gladwin and Jesse L. Nusbaum reported on a cooperative agreement between Gila Pueblo and the Laboratory of Anthropology wherein the services of Emil Haury were enlisted to conduct a field course for the Laboratory of Anthropology on the east bank of the Mimbres River. This was the Harris site where 33 pit houses were excavated. Haury had dug the Mogollon Village the previous season. The subsequent reports of these two excavations defined the Mogollon culture, something that was neither Hohokam nor Basketmaker-Pueblo.

Interest in proto-historic sites was indicated by Nusbaum and Edgar L. Hewett in describing projects in the Gobernador area, New Mexico, where Refugee Pueblo sites occur, and in excavations at Puaray and Kuaua, villages in the Rio Grande valley that probably had been visited by Coronado.

The northern limits of the Southwestern culture area were being sought in Nevada as reported by Frederick W. Hodge.

The book review section of Volume I, Number 1 of AMERICAN ANTIQUITY contained a review of the article, "A Survey of Southwestern Archaeology" by Frank H.H. Roberts, Jr., which had just appeared in the *American Anthropologist* (Vol. 37, 1935). Here was the first synthesis of the archaeology of the area to appear following Kidder's *Introduction to the Study of Southwestern Archaeology*, published in 1924. This excellent paper by Roberts summarized the history of Southwestern archaeology, evaluated the progress of investigations, and considered the cultural components of the stages established by the Pecos Conference of 1927 — the so-called Pecos Classification — for the Basketmaker-Pueblo pattern. Similarly the Hohokam, or Desert Province, was reviewed following the stages set up by the Gila Pueblo Conference — which had resulted in the Globe Classification. The Mogollon was yet to come. Roberts considered the validity of both the Pecos and Globe classifications and felt that complete revisions of them were not in order, contrary to some who favored a repudiation of both and the starting from scratch with an entirely new terminology. Roberts did offer for consideration a revision of the Pecos Classification, which has become known as the Roberts Classification, in an attempt to eliminate the chronological implications of the earlier system and to avoid the complexities caused by the absence of certain stages in some sections and the difficulty of horizon determination in others.

At about the same time the Gladwins introduced their "Method for the Designations of Cultures and Their Variations" (1934) with its system of roots, stems, branches, and phases. They retained the Pecos and Globe nomenclatures and other familiar terms for the main developments, but set up a method for making minor distinctions in the building up of cultural sequences.

Thus, we can see that the first two issues of AMERICAN ANTIQUITY in 1935 mirror most of the interests of Southwestern archaeologists at that time and hark of things to come. It should be noted here that the majority of the archaeologists who submitted copy for that report in 1935 are no longer writing of the Southwest in AMERICAN ANTIQUITY. Cummings, Hewett, Hodge, and Morris are gone; others have retired or have shifted their active interests to other areas or fields.

To move on to what has taken place since 1935, let me divide the following comments into two categories: (1) interpretation, and (2) method and theory. Here I will write of accomplishments, trends, and some problems, rather than "who dug where and what was found there."

INTERPRETATION

To be considered here are the gains in knowledge of Southwestern archaeology obtained during recent years and the interpretation of the evidence now at hand. The subject will be examined from the point of view of archaeological chronology rather than in the order in which the excavations were accomplished or the interpretations presented.

Therefore, attention is focused first on the period identified by various workers as Early Man, paleo-Indian, or the Lithic Horizon. Although recognition of this aspect of archaeology in the Southwest had been achieved prior to 1935, subsequent finds have placed it on a much firmer base. We now have better definitions of lithic assemblages, additional knowledge of their distribution in time and space, and their faunal and environmental associations. Careful excavations of habitation sites and game kill sites where lithic and in some cases bone implements were associated with extinct animal remains, and the interpretation of the contents of these sites by geologists and biologists as well as archaeologists, have led to better understanding.

The well-known pressure-flaking complexes such as Sandia, Clovis, Folsom, Eden, and Scottsbluff are generally well accepted. Refinements in point typology is illustrated by the replacing of the meaningless term "unfluted Folsom" by such types as San Jon, Plainview, and Milnesand. Hunting, especially of big game, has been revealed to have been the principle means of subsistence for the possessors of the lithic remains just mentioned, although the element of preservation may be yielding a slightly warped picture. Still we know that these Indians killed extinct forms of bison, elephant, camel, horse, and sloth with stone-tipped weapons in late glacial or early post-glacial times.

Some areas of the greater Southwest, on the other hand, are yielding archaeological remains apparently of equal antiquity to those of the Big Game Hunters but are of quite different type and function. This complex has been labeled the Desert Culture (Jennings and Norbeck 1955), and its milling stones, percussion-flaked choppers and flakers, and other artifacts support the premise that certain environmental zones of the Southwest fostered a gathering-small game hunting way of life. Some prefer to call the Big Game Hunters the Paleo-eastern tradition and the Desert Culture the Paleo-western tradition (Wormington 1957). Whatever we call them, both are represented in the Southwest, and in southeastern Arizona, the two traditions occur in the same restricted region (Haury, Sayles, and Wasley 1959).

Information on the Early Man aspect of Southwestern archaeology has reached the point where synthesis such as that presented in Wormington's *Ancient Man in North America* (1957) is possible.

Secondly, relative to interpretation, I should mention the filling of the gap between Early Man and the later sedentary cultures. In the late 1930's there was little if any archaeological knowledge of the several millennia prior to the Christian Era. The void in our knowledge was so great that it was even suggested that the Southwest was unoccupied during those times. The last 20 years have witnessed the compilation of considerable information about cultural complexes which fall within that period. Unspectacular as these remains are, for they are neither the oldest nor do they approach excellence in anything, they do establish cultural continuity. The Cochise culture was the pioneer in this field, followed by local sequences in southern California, at Ventana Cave, Arizona, on the Rio Grande, on the Uncompaghre Plateau, Colorado, and elsewhere. Now, the widespread, time-long Desert Culture is threatening to swallow all of these. These cultures, or this culture, which featured small game hunting and the gathering and processing of wild plant products, formed the base from which the later sedentary cultures evolved. A good summary of information on this period is to be found in the paper, "The American Southwest: A Problem in Cultural Isolation" (Jennings 1956).

I should mention here the changes in interpretation caused by the findings at Bat Cave, New Mexico, and other caves in that vicinity and in northern Chihuahua, which fall within this time span. Prior to digging in those caves it was a generally accepted belief that agriculture and pottery making entered the Southwest

together about the time of Christ. However, it now seems apparent that we must date the arrival of maize in the Southwest several thousand years before pottery made its appearance.

Finally, we come to the better-known sedentary cultures, or subcultures as some would prefer, the big three: the Anasazi, Hohokam, and Mogollon, and the little two: the Sinagua and Patayan.

Anasazi. My use of the term Anasazi here signifies the general acceptance of this designation for the older term Basketmaker-Pueblo. I am not sure but what we have created more problems in the Anasazi area than we have solved in the past 25 years through refinements of our concepts of culture change, regional distribution, and similar activities. An attempt was made to shift from exploration to synthesis and taxonomic improvement, but it was soon realized how little actually was known and that some areas reported upon in the early days of the Southwest needed re-examination; therefore, some attention is still being directed toward filling in blanks.

We do have a much better definition of the Basketmakers now than we had 25 years ago and of their relationships to the Pueblo. At the other end of the time scale a goodly amount of attention has been focused on connecting late prehistoric archaeology to early historic, notably by work at Awatovi in Arizona, and near Acoma and in the Rio Grande Valley in New Mexico. Reed and others have proposed links between certain prehistoric Anasazi and modern Pueblo groups which are significantly more meaningful than some of the fanciful assumptions of the Fewkes-Cushing era.

Summaries of the Anasazi, or of subdivisions of the area, have been set forth in the writings of Roberts (1937), Morris (1939), Brew (1946), and Reed (1955).

Hohokam. As mentioned earlier, this period has witnessed the "growing up" of Gila Basin archaeology. Through work principally of Gila Pueblo, the University of Arizona, and the Amerind Foundation, Hohokam culture has been better defined, its boundaries established, and certain relationships with neighboring cultures demonstrated. However, problems still remain.

Of outstanding significance during the time here considered have been the excavation of Snaketown by Gila Pueblo and the work of the Amerind Foundation in protohistoric and historic sites. Linkage of the Hohokam with present day peoples of the area has been demonstrated. Summaries of the Hohokam appear in publications by Gladwin and others (Gladwin 1937; Gladwin, Haury, Sayles, and Gladwin 1937).

Mogollon. The Mogollon is a product of the last 25 years. It is true that the Gila Pueblo surveys in southeast Arizona and southwest New Mexico beginning in 1931 had recognized a culture that seemed to be distinct from anything that had been described, but it was not until Haury completed excavations at the Harris site in 1935 that the Mogollon culture was defined. Haury's publications on the Mogollon appeared in 1936. There followed a time in which Mogollon took on many meanings to many people. From a rather nebulous beginning it has assumed some measure of concreteness principally through the efforts of Gladwin, Haury, Sayles, Martin, and Rinaldo. There has really been more digging in Mogollon sites now than has been done in Basketmaker sites, which is quite a reversal from the situation in 1935. Wheat has performed a service in bringing together many of its loose ends in his recent monograph (1955). Work is continuing in the Mogollon.

Sinagua and Patayan. Coeval with the development of the Mogollon, Colton and his associates at the Museum of Northern Arizona demonstrated through detailed analysis of remains in north-central Arizona that a number of distinctive elements were involved and had met and mixed. The Sinagua culture or branch was proposed to account for this complex (Colton 1946). Another distinctive root or basic culture, the Patayan, was established by the above group to identify the archaeological remains along the lower Colorado River and in the deserts on either side probably representing the prehistoric remains of Yuman speaking tribes (Colton 1939). These two entities have not reached the status of the big three cultures and are sometimes spoken of as illegitimate, hybrids, or transitional, or even completely ignored.

Prehistory of Athabascan tribes has received some attention, but there remains much to be desired in this area of research.

Looking at the greater Southwest as a whole, interpretation relative to local sequences and internal relationships between the sedentary groups have improved. Connections eastward

into the plains, to Texas, the Ozarks, and with the Woodland and Mississippian developments have been sought. To the west, the greater Southwest can include parts of Southern California. Northward, work in the Fremont culture and the Desert Culture is being pushed forward. It is to the south, however, that Paul Bunyan-size strides have been made. A gap, this time one between the Southwest and Middle or Nuclear America, has been closed. No longer do we have to state that "it is *believed* that the Southwest was influenced by developments from nuclear America," for we can now demonstrate that the two areas were linked. It is apparent that initially maize spread from south to north, followed by pottery making, and then by such elements as certain architectural forms, clay figurines, pottery forms and decorative styles, shell ornaments, copper bells, mosaic discs and so forth. Some of these elements may be associated with religious complexes that reached the Southwest perhaps carried by such peoples as the Toltecs. Work in northern Mexico since World War II has passed from survey and reconnaissance to excavation. Southern Illinois University, the Amerind Foundation, and the University of Colorado have been most active in this area.

General summaries for the entire Southwest include those by Roberts (1937), Judd (1940), Mc.Gregor (1941), Wormington (1947), Reed (1955), and Gladwin (1957).

METHOD AND THEORY

One of the greatest advances in methodology has been the interdisciplinary approach especially directed toward problems of chronology. Dendrochronology proved itself during this period, culminating in the work of the Laboratory of Tree-Ring Research at Tucson and the publication of the *Tree-Ring Bulletin*. Radiocarbon dating has been introduced. Archaeologists now take more advantage of the researches of physiographers, ecologists, and ethnobotanists. There has been a general maturing of our chronological thinking resulting among other things in the development of the Geochronology Laboratories at the University of Arizona.

It was a great day when the archaeologist decided that he could not solve all the problems and became willing to call in specialists. This may be regarded by some as having a splintering effect which leads to so much division of responsibility that there is not much left for the archaeologist. However, I believe the contributions from other disciplines greatly outweigh any disadvantages that may be entailed in cooperative ventures. Chances are we will have to do a lot more of this before we are really more mature in archaeology and as archaeologists. It is significant, I think, that we are beginning to focus on settlement plans and patterns in relations to land use and other natural factors. Man's relation to the land is beginning to come into its own through arid land study programs, for example.

The ordering of cultural sequences still rely to an extent upon the original Pecos Classification and its revision by Roberts and on the Globe Classification. But the Gladwin system, with modifications, is perhaps more widely employed, although it is not without its faults. It sets up broad general periods for a large part of the entire area with successive stages cutting the local sequences and each embracing phases or foci of several branches. Somewhat similar generalizing approaches also have been outlined in the "Southwest Co-tradition" of Martin and Rinaldo (1951) and in the "New Conceptual Scheme" of Daifuku (1952).

Pottery has always been of concern to most Southwestern archaeologists, and is almost worshipped on the altar by some. In the 1930's and 1940's surface surveys were carried on extensively, particularly by Gila Pueblo, the Laboratory of Anthropology, and the Museum of Northern Arizona, and various taxonomic constructs were built from them. In the 1950's, when analysis and synthesis of data already collected was attempted, it was realized that the surface surveys although useful in many ways had very definite limitations and that these limitations had been ignored in many publications of the 1930's and 1940's. Thus new areas had to be tested by excavation and previously worked areas re-examined.

Arising from all of this there is possibly coming forth a new and expanded pottery taxonomy which will provide a closer approximation to cultural entities than did the earlier classifications (Wheat, Gifford, and Wasley 1958). Leading theorists from other areas are working with Southwesterners in this connection. Also a revival of design type studies, after a period of relative quiescence, is becoming apparent. Furthermore, petrographic analysis now allows better definitions of wares.

From a taxonomic point of view, it may be noted that advances also have been made for dealing with stone and bone artifacts and with textiles and basketry through analytical studies and standardization of terminology.

A method new to American archaeology which has been foisted off on us is salvage archaeology. This is operating at full throttle in the Southwest, which is both good and bad, but it looks as if it is here to stay and we must deal with it. On the one hand participation in a salvage program frequently means setting aside our pet desires, while on the other hand it may lead to investigations in areas that might never have received attention but for such projects. Certain long term salvage programs in the Southwest, such as that of the Upper Colorado River Basin, are reaching a point where true "problem archaeology" is being accomplished. Other projects are crash programs of a strictly salvage nature. Indian land claim cases also have supported some archaeological researches. Interests of federal and state agencies, private industry, and even the American public in these projects is gratifying.

A few years ago in an inventory paper of this sort (Taylor 1954), Southwestern archaeologists were taken to task on two counts — among others. One, that they were so concerned with gap filling and so indoctrinated by a tradition of expeditionary field work that they acted as window shoppers scurrying from area to area — seldom staying in one place long enough to do a significant piece of field work. I submit that many Southwesterners, during the last 25 years and even before, have settled down to prolonged digging in one spot or area. Judd did this at Pueblo Bonito, Morris worked in the La Plata and at Aztec, Kidder at Pecos, Brew dug in the Alkali Ridge area and at Awatovi, Martin spent seasons in the Reserve area and has now moved his operation westward. Arizona worked at Point of Pines and New Mexico in the Chaco; and the National Park Service has initiated a long-range program at Wetherill Mesa in Mesa Verde National Park. These activities indicate a willingness to spend a lot of time in an area to understand it better, not only archaeologically but also in the relationship of the sites to the natural history and the physical background.

Secondly, Taylor (1954) stated that work in the Southwest was dominated by Easterners. From other sources I have heard a similar comment — that a "Harvard clique" ran the Southwest. It is true that Eastern-trained and Eastern-based archaeologists have been and still are active in the Southwest, but local Southwestern archaeologists and students are doing their share. Departments of Anthropology at the state universities of New Mexico, Arizona, Utah, and Colorado are actively training archaeologists by means of academic and field programs. All are turning out archaeological research, as are local museums and research institutions. All are publishing results of research in anthropological series.

To give an idea of training facilities at the Southwestern institutions, let me list a few combined figures for the four state universities:

Archaeologists on staffs, 1935	3
Archaeologists on staffs, 1960	13
Number of students attending archaeological field sessions between 1935 and 1960	approximately 1600
Number of students majoring in anthropology, 1935	43
Number of students majoring in anthropology, 1960	235
All four universities have graduate programs in anthropology, granting both the M.A. and Ph.D. degrees	
Number of master's degrees granted to students specializing in archaeology between 1935 and 1960	42
Number of doctor's degrees granted between 1935 and 1960	19

The last 25 years began with a period of subdivision and splitting, which divided up the material and sorted it out into small categories which could be analyzed in detail. Now Southwestern archaeology is moving into a stage of crystallization and synthesis. However, there is still need for the collection of data from some areas before comprehensive generalizations can proceed.

We still are criticized, and rightly so, for devoting too much attention to material objects and not enough to the reconstruction of culture history. Along this line I would like to call for more programs of the type sponsored by the Society for American Archaeology which resulted in the several regional seminars in 1955, including the one on the Southwest.

There exists a generally poor relationship between the amateur and the professional in the Southwest. Much unauthorized looting of

sites goes on particularly in remote areas that are being opened up to oil, gas, and mineral exploration, but such practices also occur under the very noses of scientists where perhaps a little cooperation, if possible, between amateur and professional could prevent them.

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DEVELOPMENTS IN THE ARCHAEOLOGY OF NUCLEAR AMERICA, 1935–60 *

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ABSTRACT

Archaeological developments in the zone extending from Mesoamerica to the Andes are summarized in terms of the following topics: early man, the origins of agriculture, the interrelationships of the Nuclear American cultures, the ethnic identification of archaeological complexes, horizonal and tradition formulations, the place of Nuclear America in the hemisphere, relationships between the New World and the Old World, the rise of native American civilizations, and main trends since 1935. These trends include increasing chronological control, greater awareness of context, growing interest in culture process, and more clarity and precision in definitions.

LET US EXAMINE developments in the archaeology of Nuclear America over the past 25 years of making an appraisal of the status of several major problems which confronted the archaeologist in 1935 and which still confront him in 1960. This comparison, pointing up changes in accrued data, in alignments of evidence, in interpretations, and in the phrasing of problems, will best demonstrate certain trends and their importance in the growth of archaeological science.

The major problems which I have selected are by no means the only ones of significance, but all of them are of a scope that includes Nuclear America as a whole rather than any more limited geographic frame of reference within this zone. By Nuclear America we mean the approximate center of the hemisphere along its cordilleran axis, from central Mexico down through Central America and the Andes to Peru and Bolivia. Incidentally, this concept is, in itself, one of the developments of archaeology of the last 25 years. A vast territory, Nuclear America encompasses several culture areas. It was the heartland of agriculture in the pre-Columbian New World.

EARLY MAN

In Nuclear America reliable data on the question of the earliest human occupance were almost nil as of 1935. Writing as late as 1941, Vaillant (1941:12) named his earliest horizons "Middle Cultures" in order to leave an unfilled underlying bracket for "Early Cultures" when and if they should be discovered. A few scattered finds, such as the carved Tequixquiac bone (Barcena and Castillo 1885) from the Valley of Mexico and the Punín calvarium of Ecuador (Sullivan and Hellman 1925) hinted at great antiquity but were inconclusively dated. Yet, by analogy with Early Man finds of western North America (Roberts 1935) and from inferences drawn from discoveries of early hunters at the southern tip of South America (Bird 1938), it seemed certain that evidence for Pleistocene and early Post-Pleistocene man would eventually be forthcoming from the middle latitudes of the hemisphere. Then, in a little more than the last decade, De Terra (De Terra, Romero, and Stewart 1949), Aveleyra (1950, 1956), and others, by concentrating attention on the Upper Becerra geological context in the Valley of Mexico, produced proofs for man as a big game hunter in that vicinity some ten to fifteen thousand years ago (Libby 1955, C–204, C–205). Elsewhere, in Bolivia (Menghin 1953–54; Ibarra Grasso 1955), Peru (Tschopik 1946; Cardich 1958), and Ecuador (Mayer-Oakes and Bell 1960), recent discoveries confirm the former presence of paleo-Indians in Nuclear America. The typological, geological, and chronological relationships of these Mexican and South American finds to those farther north remain to be developed; but there is no longer any question that man, as an early hunter, moved over most of the Americas during, or shortly after, the Wisconsin ice advances.

ORIGINS OF AGRICULTURE

In 1935 the earliest known archaeological horizon in Nuclear America was one of settled village farming and pottery making, and there was a tendency to consider the origin or introduction of agriculture as a rather sudden, dramatic event which had effected a sharp break with past subsistence practices and had provided the basis for a new way of life. The archaeological record as revealed in the 1940's and 1950's in Mexico and Peru has shown this concept of agricultural origins as a sudden revolution to be a distorted and telescoped view of what really happened. MacNeish's (1958) Tamaulipas cave excavations reveal, rather, that the introduction of plant domestication

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was a long, slow process, spanning several millennia and that it was paralleled by an equally slow cultural evolution. Wild seed and plant collectors and small-game hunters were transformed into sedentary villagers dependent upon plant cultivation by the gradual accumulation of such domesticates as the pumpkin, squash, bean, and, finally, maize into an agricultural complex.

Another thing which archaeologists have learned about early agriculture in Nuclear America is that there is no single center for the origin of all New World domesticated plants and, possibly, no single center for the origin of the idea of domestication. On present evidence it would appear that the pumpkin (*Cucurbita pepo*) was first cultivated in northern Mesoamerica while the red bean (*Phaesolus vulgaris*) probably had its original center in southern Mesoamerica (MacNeish 1959). The lima bean (*Phaesolus lunatus*), on the other hand, quite likely had two separate first points of domestication from separate wild races, a large variety beginning in Peru and a smaller one in Mesoamerica (P. C. Mangelsdorf 1960, personal communication). *Zea mays*, that most important American plant, while not yet tracked down to its ultimate beginnings, probably was cultivated first in southern Mesoamerica; but either it spread early to Peru or the idea of cultivating wild corn spread there, for well back in the archaeological past the majority of races of Peruvian maize differ from those of Mexico or Central America (P. C. Mangelsdorf 1960, personal communication). Lying completely outside of Nuclear America in their origins, but being brought into the Andean areas at a fairly early time, are such important crops as manioc and the peanut, which were first cultivated in the tropical lowlands of eastern South America (Sauer 1952, 1959).

The picture of the rise of Nuclear American farming which emerges from all of these data is that of a pattern slowly forming through time. Experimentation with plants and an improvement of them, together with their diffusion and cross-diffusion within the Nuclear American zone, appears to have lasted from 7000 to 1500 B.C. in Mesoamerica and from at least 2000 to 1000 B.C. in Peru (MacNeish 1958; Bird 1948; Willey 1960a). This era of experimentation and dissemination—or stage of "incipient agriculture"—was terminated by the attainment of the threshold of village agriculture. It may be that then, explosively, the new, fully-formed subsistence pattern spread from some one center within Nuclear America; or perhaps there were several such centers responsible for the propagation of an "established" agricultural way of life (Willey 1960b). This we do not know, and it remains a major next step in our learning about the origins of agriculture in native America. The question also leads to the following problem to be considered.

Interrelationships of Nuclear American Cultures

Spinden offered the first comprehensive explanation of the similarities among the pre-Columbian cultures of Nuclear America. This was his "Archaic hypothesis" by which he maintained that sedentary village farming first originated in the New World in or near the Valley of Mexico and that it spread, along with certain pottery and figurine types, from central Mexico to Peru and beyond (Spinden 1928: 45–71). In this way a platform was provided for the subsequent development of the American civilizations whose basic content and orientations were derived from this commonly shared understructure. The oversimplicity of the Archaic hypothesis was challenged in the early 1930's when it became evident that monogenesis in, and diffusion out of, the Valley of Mexico would not account for all of the early pottery traditions of Mesoamerica, let alone the entire New World (Merwin and Vaillant 1932: 64–5). Nevertheless, shorn of its somewhat parochial qualities, Spinden's core idea remained a part of the archaeological thinking of the times; and both Kroeber (1930) and Kidder (1936) predicted that the interrelationships between Mesoamerica and Peru, as well as the involvement of the area in between, would be clarified when archaeologists had worked their way back in culture sequences to the beginning of the first millennium before Christ.

These predictions of Kroeber and Kidder have, indeed, been verified on just this early level of about 1000 B.C. Strong (1943) was one of the first to point to highly specific ceramic resemblances between Mesoamerica and Peru in this early period. Following this, the discovery of the Tlatilco culture, in Mexico, and the Tlatilco pottery linkages to the Chavín styles of north coast of Peru gave added proof of these inter-area ties (Porter 1953; Willey 1955a, 1955b). Still later, with the revelation

of such phases as the Colombian Momil (Reichel-Dolmatoff and Reichel-Dolmatoff 1956), the Guatemalan Ocós (Coe 1959), and the Ecuadorean Valdivia and Chorrera (Estrada 1956, 1958; Evans and Meggers 1957, 1958), the strategically intermediate pieces of the puzzle are falling into place (Willey 1959). At the present writing, this forging of a chain of mutually related early village agricultural complexes, from central Mexico to south Peru, is being completed in Costa Rica (Coe 1960; Lothrop 1960). The ceramic features shared by these early Nuclear American cultures are interesting ones. Both specific decorative techniques and vessel forms are held in common. Among these are combined incision and rocker-stamping modes of ornamentation, red-painted bands outlined with incision, negative or resist painting, and complex vessel shapes such as the stirrup-mouthed jar. Mexican-Peruvian relationships are further indicated on this early chronological level by the common possession of such concepts as the dual being represented in pottery by vertically divided half-living and half-death's-head faces or similar half-human and half-animal beings (Covarrubias 1957, Pl. 4; Larco Hoyle 1941, front cover).

For the later pre-Columbian periods, those corresponding to the Classic and Postclassic periods of Mesoamerican and Peruvian chronologies, or the last 1500 years before the Spanish Conquest, it has been speculated that sea travel might have been responsible for certain Mexican-Ecuadorean parallels in pottery figurines and three-pronged incense-burners (Borhegyi 1959). Quite possibly metallurgy, which is indisputably a single Nuclear American technical tradition, was passed in this way from Peru and Ecuador to western Mexico.

Although relative dating gives some clues as to the direction of movement of certain traits within the Nuclear American zone—and metallurgy is a case in point—the areas and regions of origin of some of the most basic elements are still unknown. Where, for instance, did pottery first appear in Nuclear America, and what were the routes of its diffusion? And within a still wider frame of reference, has Nuclear American pottery any relationships with the early pottery of woodland North America?

Ethnic Identifications

The identification of archaeological complexes as the remains of historically known tribes or nations has always been a major concern in Americanist studies, and it continues to be. Certain contributions have been made on this score in recent years. Outstanding among these is the documentation of Tula, in Hidalgo, as the famed "Tollan" of the Toltecs. Until the early 1940's it was generally conceded that the site of Teotihuacán was the ancient "Tollan" (Vaillant 1941); but with Acosta's (1940, 1944, 1945) excavations at Tula, it became evident that the latter site was the true Toltec capital (Armillas 1950: 64–6). This identification of Tula with the Toltecs has had far-reaching influences on archaeological interpretations in Mesoamerican prehistory. The close similarity of the Tula sculptures and those from Chichén Itzá, in Yucatán, leaves no doubt as to the cultural affiliations, if not the ethnic identity, of the early Postclassic period invaders of Mayan Yucatán. Charnay (1887: 75–103) identified Tula with the Toltecs and even called attention to Tula–Chichén Itzá resemblances in stone sculptures. Tozzer (1930) also identified certain sculptured and painted figures at Chichén Itzá as Toltec warriors. It was not until the excavations at Tula, however, that the Tula-Toltec affiliation was established.

In South America the extension of Inca origins back in time, to a provincial, pre-imperial Killke period and archaeological complex has been one of the most notable achievements in ethnic identifications (Rowe 1944: 60–2). Another significant line of research in Peru has been in the fine sequence calibrations made on the south coast as a result of a combined archaeological and ethnohistorical approach. These studies offer an extremely interesting insight into the kinds of ceramic stylistic changes which were brought about on a local Ica pottery-making tradition by the subsequent Incaic and Spanish conquests (Menzel 1959).

Horizonal and Traditional Formulations

As early as 1903 Uhle recognized the area-wide, or near area-wide, distributions of Peruvian pottery, sculptural, and textile styles. These distributions and styles were defined more fully by Kroeber (1944) who, along with Bennett (1943), added a Chavinoid horizon style to the previously recognized Incaic and Tiahuanacoid horizons. Willey (1945) noted two other Peruvian horizon markers, the White-on-red and the Negative-painting, and also suggested the complementary phenomenon to the

horizon style, the pottery tradition through time. Bennett (1948), expanding on this latter concept and concerning himself with the problem of culture continuity within a given geographic area, advanced the idea of the "Peruvian Co-Tradition." The co-tradition formulation had a predecessor and a partial counterpart in the Mesoamerican culture-area-with-time-depth which Kirchhoff (1943) described. For Mesoamerica, Parsons (1957) has made a survey of horizon styles and markers on the Peruvian model. Although the horizon and tradition concepts are essentially historical devices, most useful for regional cross-dating and for delineating relationships through time, they have also opened the way to an examination of culture contact (Lathrap 1956) and culture continuity phenomena (Thompson 1956) from the archaeological point of view.

The Place of Nuclear America in the Hemisphere

Within the larger New World setting Nuclear America generally is considered the primary diffusion center for agriculture, certain pottery traditions, and other traits associated with a sedentary way of life. The North American Southwest and Southeast and the Tropical Forest and South Andean areas of South America are seen as peripheral to this center. On occasion this view has been called into question, as, in the 1930's, when Gladwin, seeking Hohokam and Mogollon origins and finding no proper Mexican prototypes for his red and red-on-buff pottery argued for a north-to-south spread of such ceramics as a part of an Asiatic-derived "Cordilleran Complex" (Gladwin 1937: 87). Gladwin (1948: 238) has since withdrawn his opinion of the northern derivation for the early Mogollon red pottery.

Since that time, however, evidence supporting the older hypothesis has been detailed by Ekholm (1942: 133–6), Haury (1945), J. C. Kelley (1956, 1960; Kelley and Winters 1958), Di Peso (1956: 562–4), Lister (Lister and Howard 1955), and others. Ceramics, ball courts, and a good many additional traits of the Chalchihuites culture of Zacatecas and Durango have been matched with those of Hohokam to effect a series of cross-ties beginning between A.D. 500 and 900. These dates represent the range of opinion for the beginnings of Mexican traits in Hohokam. Haury favors the earlier date; Di Peso, the later. Furthermore, the antiquity of red-on-buff wares still farther to the south, in the Valley of Mexico, leaves little doubt as to the south-to-north movement of these traits (Tolstoy 1958: 26). A number of important questions concerning Mesoamerican-Southwestern relationships remain open, however. Among these is the diffusion-versus-migration argument as this involves the transmission of Mesoamerican influences. Another is the still missing series of pottery finds for north Mexico that would relate early Mogollon red ware to the south, presumably at a time well before this rise of the Chalchihuites culture.

Mesoamerican influences into the Southeastern United States still lack sharp temporal and culture-complex definitions. A consensus would relate the Mississippian and Temple Mound cultures to Mesoamerica, but a more specific dating of these influences and the particular Mexican sources of origin remain uncertain. On earlier time levels the degree of Mesoamerican influence on the Burial Mound cultures is widely debated. In sum, the interpretations of the relationships between Mesoamerica and the Southeast have not changed radically since Phillips (1940) and, again, Griffin (1949) dealt with the subject.

In South America a good start has been made toward relating the lowland tropical forest cultures to Andean Nuclear America. In the north, in Colombia, the earliest complexes may turn out to have had close ties with the eastern lowlands and to have been based on vegetative rather than maize agriculture (Reichel-Dolmatoff and Reichel-Dolmatoff 1956: 269–77; Cruxent and Rouse 1958, Vol. 1: 263–5); in Ecuador Meggers and Evans (1958) believe they have clues to indicate an Andean-to-lowland current of influence; and in eastern Peru the earliest known archaeological levels have pottery which resembles that of Andean Peru of the first millennium B.C. (Lathrap 1958). The one main fact that we can be sure of is the Nuclear American–tropical forest relationships were not a relatively late pre-Columbian development but go back, at least, to B.C. times. At the southern end of Nuclear America a recent spate of archaeological research by Rydén (1947, 1952, 1957, 1959) and by Ponce Sangines (1957) and associates has afforded some idea of how Tiahuanacoid and pre-Tiahuanacoid influences from the Bolivian altiplano penetrated to the east and south.

The New World and the Old

The great question as to how, when, or if, the ancient civilizations of the Far East or Southeast Asia influenced those of Nuclear America still stands much as it did in 1935. Or, at least, this is my opinion. Heine-Geldern's (1954) scholarly article on the possible diffusion of metallurgy from across the Pacific to Peru, with the apparent synchronization of events, is one of the most effective presentations for the trans-Pacific point of view to date. This, together with his and Ekholm's observations on Indian and Chinese art styles and architecture vis-a-vis Chavín, Totonac, Olmec, Maya, and Marajoara are certainly provocative (Heine-Geldern and Ekholm 1951; Heine-Geldern 1959a, 1959b, 1959c; Ekholm 1953). But on this problem I am always impressed with how little, if any, new evidence ever appears. Americanists have been digging rather intensively in lots of places in the last 25 years, but no object or item is ever exhumed which bolsters the trans-Pacific case. Maybe it is an instance of "none is so blind as he who will not see," but I cannot go beyond the Scot's verdict of "not proven" as of 1960. At the meeting at which this paper was first presented (May 6, 1960), Emilio Estrada, Guayaquil, Ecuador, informed me that pottery head-rests, pottery house models with upcurved roof-peaks, and the custom of placing a series of pottery tubes from ground surface to the face of the dead in the grave are found as a complex on the north coast of Ecuador. This complex does not date from the earliest Ecuadorean pottery horizon but is tentatively placed at about the beginning of the Christian era. Estrada further informed me that all of these traits are to be found in the archaeology of certain east Asian regions. I will comment no further, awaiting Estrada's publication of these findings; however, from the information given these discoveries may have great importance for the problem of trans-Pacific relationships between the Old and New World civilizations.

The Rise of Native American Civilizations

This last of the major problems is, in a sense, the sum total of all the others. Here, however, I wish to comment only upon certain aspects of it.

American archaeologists have been increasingly interested in the way the Nuclear American civilizations developed. This interest has led to attempts to characterize civilizational growth by diachronic classifications. Among some of these classifications are those of Larco Hoyle (1948), Strong (1948), Bennett (1948), and Willey (1948) for Peru and of Armillas (1948) and Caso (1953) for Mexico. These classificatory schemes proposed concepts and terms such as "Formative," "Classic," "Florescent," "Militaristic," and "Expansionistic" for their sequential divisions. In part, the schemes have been used as area chronologies; and, in part, they are hypotheses about evolutionary and configurational changes in culture and societal types through time. They have been controversial on both scores, particularly the latter. It is now obvious that general area chronological schemes are necessary means of integration and comparison. At the same time it is likely that: (1) a period terminology which does not carry developmental or configurational connotations will be selected for these area chronologies or, (2) that present terminology will gradually become devoid of meaning other than that of a purely chronological kind.

I do not think that this means that the developmental classifications have had a negative effect upon Nuclear American archaeology. It is simply that chronology and qualities of culture change are best treated and studied separately. Developmental classifications have contributed importantly to our thinking about the nature and the growth of civilization. Hypotheses about these subjects are, of course, arguable; but the framing of problems and the search for data pertinent to the testing of such hypotheses have led to an enrichment of the archaeological record that is generally welcome. For example, in asking the question of what are the crucial limiting factors of natural environment in the development of civilization attention has been turned to cultural-environmental relationships. Similarly, by inquiring into the social setting of civilization and by attempting to pose definitions of villages, towns, or cities the heretofore neglected dimension of archaeological settlement patterns has become of prime interest.

Main Trends Since 1935

Glancing back, four main trends stand out in the development of Nuclear American archaeology over the period 1935 to 1960. They are not unique to Nuclear America or to American archaeology. Without attempting to trace their intellectual history it can be said that all four are as old as the first interests in archaeology;

however, as this review of major problems has shown, their emphasis is relatively recent. See also Kidder (1958) for a review of developments and trends in Mesoamerican archaeology for the period 1906-56. The reader should also refer to Griffin (1959) for a summation of trends in North American (United States) archaeology over the past 30 years.

The first trend is that of increasing chronological control over archaeological data. In 1935 stratigraphy as a way to relative chronology in Nuclear America was known largely from the works of two men, Vaillant in Mexico and Bennett in Peru. Two-and-a-half decades later it is taken for granted as a standard archaeological operation. Although there had been some published stratigraphic observations in the Valley of Mexico as early as 1911, Vaillant's excavation in 1930 of the Zacatenco site marks the real beginning of modern stratigraphic archaeology in Mesoamerica (Vaillant 1931). Similarly, in Peru, Uhle (1903) had carried out stratigraphic work in the sense of architectural and grave superpositions; but Bennett's (1934) Tiahuanaco study is a milestone comparable to Vaillant's.

In addition, new methods of dating have been introduced from the physical and chemical sciences. Radiocarbon readings have pushed dating estimates of the incipient agricultural and early village agricultural horizons back several millennia before the Christian Era. Regrettably, several important special problems in chronology are yet unsolved. Outstanding among these is the Maya-Christian calendrical correlation. Perhaps radiocarbon, or some other chronological aid, will help us settle this in the next 25 years. Satterthwaite and Ralph (1960) have already provided strong support for a radiocarbon verification of the 11.16.0.0.0, or Goodman-Martínez-Thompson correlation.

A second trend might be described as a greater awareness and appreciation of context. Context can mean anything from the significance of foreign trade pottery in a sealed tomb to the fact that an archaeological site represents a small satellite village within the orbit of an urban-centered civilization. Similarly, the reconstruction of past climates and landscapes, the interpretation of cultural-environmental relationships from settlement patterns, or a consideration of social and political settings from a variety of evidence are realized through context. It is agreed that the single archaeological datum is nearly meaningless without context.

The third trend is the growing interest in culture process. The archaeologist in Nuclear America, as elsewhere, is less than satisfied to find out "when" and "where"; he has also asked "how" and "why" questions. That his success in answering such questions is debatable is not to deny the pervasiveness of the trend. It may be expressed in hypotheses about the collapse of civilizations, the developmental rhythms of culture change, or the mechanisms of diffusion.

A fourth trend is, perhaps, the most important of them all. It overrides and cross-cuts the others. This is the trend toward clarity and precision in definitions. It may pertain to definitions of any kind, from pottery types to culture areas. Such a trend is a concomitant of the onset of maturity in any science. I hope I am not over-optimistic in seeing it in archaeology. It is certainly the one trend that meets with unanimous approval although it, too, has its drawbacks.

So far, I have looked on the sunny side. Are there no omissions, failures, adverse trends? The misgiving that comes most readily to mind is that we have been remiss in our interest in, and study of, culture content. Content is not quite the same thing as context, although it is closely related. The difference is in point of view. For instance, there seem to be fewer of us now who are interested in the creations and institutions of the Nuclear American civilizations in themselves rather than in the way these things came into being. Or maybe I should correct this to say that there are *relatively* fewer of us. There are more archaeologists now, and most of us do not control such things as Maya hieroglyphics, possess an intimate knowledge of the great art styles, or know our way about in the early source material of Mexico and Peru. Still, in absolute numbers the outlook is not so bad. In their consecutive times there was one Markham and one Means for Peru; today there is one John Rowe. In hieroglyphics and calendrics Thompson, Satterthwaite, Caso, Barthels, and Zimmerman carry on for Forstëmann and Goodman, and there are also a number of others devoted to Mayan and other native Mesoamerican writings and calendars. In art, Proskouriakoff keeps up the high standards of analysis and description that were set by Spinden.

This brings us to a good point upon which to close. If I had to predict directions for the fu-

ture in Nuclear American archaeology, I would guess that, along with the continued systematic spatial-temporal organization of data, the most rewarding trend of the next 25 years will be toward a greater verisimilitude in reconstructions and interpretations that results from an improved understanding of both context and content.

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HARVARD UNIVERSITY
Cambridge, Mass.
January, 1961

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ARCHAEOLOGY IN LOWLAND SOUTH AMERICA AND THE CARIBBEAN, 1935–60 *

IRVING ROUSE

ABSTRACT

Archaeological research in those parts of South America not included in Nuclear America is reviewed with an emphasis on Amazonia and the Caribbean area. Southern South America is primarily important at present for the well documented series of Early Man sites which indicate that American Indians had reached the farthest periphery of the hemisphere by as early as 7000 B.C. There have been three main developments and one shortcoming in the archaeology of Amazonia and the Caribbean area during the past 25 years. The developments are the discovery and formulation of new cultural complexes or phases, the establishment of chronological control which has been most successful in the Caribbean area, and the introduction and archaeological testing of Steward's concept of evolutionary levels correlated with environment, especially in Amazonia. The shortcoming is the lack of interest in reconstructing the culture of the excavated sites.

THIS PAPER is intended to cover all of lowland South America, excluding only the parts of the continent discussed by Willey (1961). However, I shall say only a few words about the southern part of the continent, since my time is limited.

Perhaps the most important development in the south during the past 25 years has been the discovery of Early Man sites, first by Junius Bird (1938) and later by the Argentines, Menghin (1952) and González (1952). These sites have been well dated, both through geological correlations and by means of radiocarbon analysis, and therefore provide powerful support for conclusions concerning the antiquity of man in North America, since they indicate that the American Indian had reached the farthest periphery of the hemisphere as early as 7000 B.C.

Work on the later remains in southern South America seems to justify the term "Marginal" which ethnologists (such as Cooper 1942) have applied to the area. Bird's sequences, for example, show that the early hunting cultures survived with relatively little change into the historic period. Only in northwest Argentina do we find strong influences from Nuclear America. Here, I think that the major development of the past 25 years has been the work of Wendell C. Bennett and his students at Yale in seriating the relatively well-documented finds of the Argentine archaeologists to establish a three-fold sequence of chronological periods, preceding the Inca conquest (Bennett, Bleiler, and Sommer 1948). This seriation has recently been confirmed and refined by González (1959), who has been able to show that bronze goes back to A.D. 800, the earliest date known for it in the New World.

Turning to southern Brazil, I should mention the local excavations during the 1930's at Confins Cave, in the Lagoa Santa region, which was hailed at the time as another find of Early Man, the skeleton obtained there being compared with Bird's material from Patagonia. Recent excavations by Wesley R. Hurt (1960) in the Lagoa Santa region cast doubt upon the antiquity of this find.

The great shell heaps, or *sambaquís*, of the coast of Brazil continue to attract attention. It has now been definitely established that many of these heaps are of human origin and that they date from more than one period of time. Serrano (1946) and Willems and Schaden (1951: 146–51) postulate successive preceramic and ceramic occupations, the latter possibly resulting from a southward expansion of the Guaraní Indians, but their sequences are not yet soundly based upon stratigraphy.

I come now to the two parts of lowland South America which seem to be worth more extended discussion: Amazonia and the Caribbean area. By Amazonia is meant the drainage of the Amazon River, extending from the so-called Montaña, or eastern slopes of the Andes, in the west to the Atlantic coast on the east, and from the central plateau of Brazil, which has just been discussed, on the south through the Guiana highlands and coast on the north. The Caribbean area consists of the basin of the Orinoco River in Venezuela, the adjacent coastal areas along the Caribbean Sea, and the islands off shore, including the West Indies. The land along the western side of the Caribbean, in Colombia and Central America, will be arbitrarily excluded, except insofar as it figures in the over-all syntheses, since it has been covered in the paper on Nuclear America (Willey 1961).

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At the time the Society for American Archaeology was founded in 1935, Amazonia was one of the most poorly known areas of the world, archaeologically speaking. Some collections had been assembled and published, notably from Marajó Island at the mouth of the Amazon and from Santarem, partway upstream, but to my knowledge the only attempt at systematic excavation had been Nordenskiöld's work in the Montaña of Bolivia in 1913–14. Nordenskiöld (1930) had also published a brief resumé of the archaeology of the area, in which he touched upon two aspects of interpretation which were to dominate the thinking of subsequent archaeologists, the identification of linguistic stocks, or local tribes, and the use of these identifications to trace widespread movements of people through the Amazon valley.

The Caribbean area, as it is defined here, was much better known in 1935, thanks to the research programs carried out there by J. Walter Fewkes, of the Bureau of American Ethnology, and by various archaeologists working for the Museum of the American Indian, Heye Foundation in New York (for example, Fewkes 1922; Harrington 1921). This work, however, was largely descriptive and lacked time depth. The principal syntheses, by Gower (1927) and Lovén (1935), consisted of studies of the distribution of traits, in an attempt to show diffusion of culture between South and North America via the West Indies. In addition Lovén, like Nordenskiöld in Amazonia, tried to identify historic tribes and to trace their origin through archaeology. Between the two, Nordenskiöld and Lovén had the Arawak migrating from the Montaña down the Amazon, through the Casiquiare Canal to the Orinoco, and thence out into the Greater Antilles.

In 1933–34 Wendell C. Bennett, assisted by Junius Bird, excavated a series of sites in lowland Bolivia, following up the work previously done there by Nordenskiöld. Since this work was not published until 1936, it may be considered the first major development within the period with which we are concerned. Its chief significance was chronological. Utilizing both stratigraphy and cross-ties with the archaeology of the highlands to the west, Bennett (1936: 401–12) was able to establish a sequence of Early, Middle, and Late periods, conforming to the chronology current at the time in Peruvian archaeology.

In 1934–35, Froelich Rainey excavated in Puerto Rico in connection with the newly established Caribbean Anthropological Program of Yale University. Once again, the primary significance of the work was chronological; Rainey (1940) demonstrated the existence of two ceramic periods, which were subsequently expanded to three by my work, corresponding to Bennett's three-fold sequence for lowland Bolivia (Rouse 1952). Further excavations by Rainey, Osgood, and others connected with Yale's Caribbean Anthropological Program, led to an extension of this sequence to Haiti, Cuba, the Virgin Islands, Trinidad, and Venezuela; and to the addition of a fourth, preceramic period at the base of the sequence. (For a bibliography of this work, see Mintz 1960.)

In 1946–47, George Howard, a graduate student at Yale, was encouraged by Bennett to attempt a chronological synthesis of Amazonia. Bennett reasoned that the sequence of three ceramic periods was by now well established for the Caribbean area, that it was also demonstrated for lowland Bolivia, and that it could logically be extended to the intervening parts of Amazonia, including the collections from Marajó and Santarem. Howard proceeded to do so in his doctoral dissertation, which was published at Yale (Howard 1947).

The climax to this type of research came during the past two years with the publication by Cruxent and Rouse of an *Archeological Chronology of Venezuela* (1958–59). The same four-fold sequence is used here, a preceramic period followed by three ceramic periods. This sequence is correlated with Reichel-Dolmatoff's (1954) chronology for the north coast of Colombia, as well as with the previously established Antillean chronology (Rouse 1951).

Howard's synthesis of the chronology of Amazonia must be considered unproved, since we still lack an adequate number of local stratigraphic sequences, particularly for the key central part of Amazonia. The Caribbean chronology is much sounder, for it is based upon a large number of local sequences and they are well distributed throughout the area. All available chronological techniques have been used in establishing these sequences: stratigraphy; seriation; cross-dating by means of European and Indian trade objects; correlation with geological events, such as changes in the sea level; and, most recently, radiocarbon dates. Thanks mainly to the Geochronometric Laboratory of

Yale University, we now have a series of over 30 radiocarbon dates, which indicate that intensive agriculture goes back to at least 800 B.C. in the Orinoco Valley of eastern Venezuela, almost as early as in Nuclear America.

A by-product of this chronological research has been the recognition of certain widespread regularities in the distribution of ceramic traits which may be subsumed under the term "tradition," "horizon," or as Cruxent and I prefer to call them, "series." Most of these series appear to be the result of diffusion or persistence of ceramic traits, but a few seem to indicate migration of peoples, for example, the spread of white-on-red Saladoid pottery from the Orinoco Valley to the north coast of Venezuela and thence out into the Antilles (Cruxent and Rouse 1958–59, Vol. 1: 244–5). Such theories of migration should be more reliable than the earlier ones, like Nordenskiöld's, which were based upon non-chronological data and upon much more spotty distributions.

While these chronological developments were taking place, a different trend in archaeological interpretation had arisen under the leadership of Julian Steward (1947) who introduced the concept of levels of cultural development, with which he had been working in Nuclear America. He defined three levels, Marginal, Circum-Caribbean, and Tropical Forest. The first of these refers to hunting and gathering peoples, such as still survive in southern South America. By the Circum-Caribbean level is meant agricultural Indians with well-developed chiefdoms and priest-temple-idol cults. The Tropical Forest level has agriculture, but lacks developed chiefdoms and cults.

In addition to defining these three levels, Steward (1947) worked out a hypothesis of culture history to explain how they came into existence. He postulated that the first inhabitants of both Amazonia and the Caribbbean area were on the Marginal level of development. Subsequently, Indians on the Circum-Caribbean level moved down out of the Andes of Colombia, and occupied the northern coast of South America. When they reached the mouth of the Orinoco, they split, one group going out into the Antilles and the rest continuing on along the coast through the Guianas to the mouth of the Amazon and up that river to seize most of Amazonia from its Marginal inhabitants. There was supposedly a parallel movement up into the Orinoco Basin. Steward further theorized that the climate in the Caribbean area and Amazonia was so unfavorable as to cause most of the Circum-Caribbean peoples to abandon their chiefdoms and cults, that is, to regress from a Circum-Caribbean to a Tropical Forest level of development. Only in the regions which were environmentally most favorable, such as the Greater Antilles, did Circum-Caribbean culture survive until historic time.

In 1948–49, two of Steward's students, Betty J. Meggers and Clifford Evans (1957), set out to test his theory by excavating at the mouth of the Amazon and in adjacent Brazilian Guiana. They were able to establish a detailed local chronology, using the techniques of seriation worked out by James A. Ford (1949), and they found that, contrary to Steward's theory, Tropical Forest cultures preceded the local Circum-Caribbean phase, known as Marajoara. Hence, they had to modify the theory, although they retained its stress upon the limiting effect of environment. They concluded that the mouth of the Amazon was originally inhabited by people on a Tropical Forest level of cultural development. At a late date, the Circum-Caribbean people of the Marajoara culture migrated down the Amazon and took over part of the area from the Tropical Forest people. These Circum-Caribbean people were unable to survive because the environment was unfavorable; and after a relatively short time, they abandoned the area to the indigenous Tropical Forest tribes.

Meggers and Evans (1958) subsequently excavated in the Montaña of Ecuador to obtain evidence that the migration which brought the Circum-Caribbean Marajoara culture to the mouth of the Amazon had originated there. They also attempted to trace one of the Tropical Forest phases at the mouth of the Amazon northwards into British Guiana, but were not successful in this (Evans and Meggers 1960).

Other archaeologists, working in the Caribbean region, have likewise found conflicts between Steward's theory and the facts of the archaeology. For example, I have pointed out that Tropical Forest culture precedes Circum-Caribbean culture in the Greater Antilles instead of degenerating from it, as Steward had thought (Rouse 1953). Similarly, Reichel-Dolmatoff (1958: 483) has shown that Tairona, the Circum-Caribbean culture of northern Colombia, is protohistoric, and that it moved up

into the mountains from the coast, instead of descending from them, as Steward's theory assumes.

Recent discoveries along the Pacific coast of Central and South America have also proved to be inconsistent with the theory. They indicate that the earliest Formative culture, which Steward considered to be the source of Circum-Caribbbean culture, was situated not in the highland but in the tropical forests along the coast (Evans and Meggers 1957; Coe 1960). It would thus appear that the highlands of the Northern Andes lagged behind the coast during the period when Steward postulated the rise of Circum-Caribbean culture there, instead of being a center of development at that time, as he thought.

These facts and conclusions have led Cruxent and me (1958–59, Vol. 1: 263–5) to reject Steward's theory and to fall back upon one previously proposed by Osgood and Howard (1943: 5), who postulated the existence of two north-south lines of diffusion, one through western and the other through eastern South America. We had obtained evidence that there were separate centers of ceramic development in western and eastern Venezuela, which presumably arose out of the western and eastern lines of diffusion respectively. Following Sauer (1952), we suggested that these are to be correlated with different forms of agriculture, the Formative type in the west emphasizing maize and a Tropical Forest type in the east relying mainly upon manioc.

Before leaving the tripartite distinction between Marginal, Tropical Forest, and Circum-Caribbean, I should note that some confusion has resulted from an alternative use of these terms to refer to culture areas rather than to levels of development. Indeed, the terms were first coined by ethnologists to refer to culture areas. I have already mentioned that Cooper and others applied the term "Marginal" to southern South America, where the early hunting form of life survived until historic time. Steward (1946–59) subsequently adopted it for the sub-title of Volume 1 of the *Handbook of South American Indians*, dealing with the tribes of southern South America, and coined the terms "Tropical Forest" and "Circum-Caribbean" for the sub-titles of Volumes 3 and 4, referring respectively to the tribes of Amazonia and to the tribes around the Caribbbean Sea (excluding those in El Salvador, Guatemala, British Honduras and Honduras, which belong to Mesoamerica). He continues to use the terms in the same way in his most recent publication (Steward and Faron 1959).

In proposing the concept of a Circum-Caribbean area, Steward (1946–51, Vol. 4: 2) gave credit to Kirchhoff for suggesting that the entire Caribbean forms a single cultural unit. However, Kirchhoff (1943, 1960) has consistently maintained in his own publications that the eastern and the western parts of the Caribbean form separate cultural units; he groups the eastern part of the Caribbean with Amazonia and assigns the western half to a "Chibcha superarea" (Kirchhoff 1943: 100). The concept of a Circum-Caribbean culture area also conflicts with the current archaeological practice of including the western half of the Caribbean within Nuclear America as an "Intermediate area" (Haberland 1959), a practice which was followed in the organization of the present symposium. Finally, the concept is negated by the Cruxent-Rouse evidence of separate centers of cultural development in Venezuela, as already discussed, for the eastern center is related to Amazonia, the Guianas, and the West Indies, whereas the western center is culturally a part of the Intermediate area of Nuclear America.

For all these reasons, it is suggested that the term "Circum-Caribbbean" be restricted in usage to its developmental meaning. From the standpoint of culture areas (in archaeology at least) it is preferable to recognize two separate units: a Caribbean area, comprising the lands on the eastern side of the Caribbean Sea; and the Intermediate area, taking in the lands on the western side of the sea.

Steward (1947) has raised the question whether Circum-Caribbean culture, in the developmental sense, may not have influenced the Indians in the southeastern United States. In so doing, he resurrected the earlier theories of Gower, whom I mentioned as an example of distribution study in the 1920's. Recent research does not support these theories. William C. Sturtevant (1960) convincingly demonstrates that the similarities cited by Gower and the authors who followed her are either erroneous, so superficial as to be meaningless, or else attributable to parallel development. In a paper on the distribution of the three levels of cultural development in the Caribbean area, I have shown that Circum-Caribbean culture in

the Greater Antilles is in fact later than the same level in the Southeast, so that the latter could not have been derived from the former, as Steward suggested; and that there is a geographical gap of several thousand miles between them, which it is difficult to reconcile with theories of diffusion (Rouse 1958).

Gower (1927) and Lovén (1935), among others, had also called attention to the possibility of connection between Mesoamerica and the Antilles. Little has been done with this hypothesis in recent years, except for Ekholm's demonstration that the so-called stone collars and elbow stones in Puerto Rico may have had the same function as the stone yokes and palmate stones in Mexico and that therefore the former may be derived from the latter (Ekholm, MS).

Returning from the rarified atmosphere of theory to something more substantial, I should mention Cruxent's discovery of the El Jobo complex in western Venezuela. El Jobo is the first paleo-Indian complex to be found in northern South America, is solidly documented by material from over 40 sites, and has a radiocarbon date of 16,000 years ago, although this is unreliable, since it comes from a disturbed site (Cruxent, personal communication).

Also noteworthy is the great increase during the past 25 years in our knowledge of the "meso-Indian" inhabitants of the Caribbean area, that is, of the non-agricultural, non-ceramic peoples who deposited their remains in shell heaps along the shores, paralleling the *sambaquis* of the Brazilian coast, to which reference has already been made. Whereas only a single meso-Indian complex was known in 1935, we now recognize 15 of them and have obtained a series of radiocarbon dates going back to 2200 B.C. in Venezuela (Rouse 1960). There has been a comparable increase in the number of neo-Indian, or ceramic, phases defined for the Caribbean area.

Finally, I should mention a significant shortcoming in the archaeology of the past 25 years. Little attempt has been made to work out the ways of life of the people who inhabited the sites, as opposed to their cultural complexes, chronology, and prehistory (compare Taylor 1948). To be sure, it is exceedingly difficult to do this in Amazonia and the Caribbean area, because the complexes or phases are so simple and so much of the material culture is perishable. Even settlement patterns are difficult to discern (Meggers and Evans 1946: 156). Nevertheless, more could have been done with this method had there been more interest in it. For example, I do not know of a single reasonably complete excavation of a site during the past 25 years, in either Amazonia or the Caribbean area. (Mason's excavations at Capá in Puerto Rico, although published in 1941, were undertaken well before 1935.) All of us have sampled only enough of our sites to determine their cultural complexes and chronological positions. Similarly, there have been few attempts to work out the functional implications of the artifacts. (Osgood 1942: 33–9 is an exception.) Some of us have applied the direct historical approach in an attempt to carry ethnological traits back into the archaeological past, but this is likewise difficult, partially because of the rudimentary state of our knowledge of the ethnohistory and partially because the historic sites are difficult to identify (Meggers and Evans 1946: 156–7).

In summary, it seems to me that there have been three main developments and one important shortcoming in the archaeology of Amazonia and the Caribbean area during the past 25 years. The first development is the discovery and formulation of new cultural complexes or phases. In 1935, this type of approach was only beginning, but in the Caribbean area we now recognize as many complexes or phases as in any other comparable part of the Americas.

The second development has been the establishment of chronologies with which to date the complexes. In Amazonia, these are still limited to isolated peripheral areas—the mouth of the Amazon, British Guiana, lowland Bolivia, and I should also mention the recent work by Lathrap (1958) in the Montaña of Peru. In the Caribbean area, on the other hand, the establishment of local sequences has been so widespread that we are now able to synthesize them into a single, relatively detailed and highly complex chronology covering the entire region, to delineate series of ceramic styles within this chronology, and to infer diffusion and migration from the series.

A third development has been Steward's introduction of the concept of evolutionary levels and his attempt to correlate these levels with differences in the environment. Evans and Megger's modification of Steward's approach is undoubtedly an improvement, and it is to be hoped they and others will continue to test the approach by further work in other parts of Amazonia.

The shortcoming consists of the failure of Amazonian and Caribbean archaeologists to do as much as they might have done to work out the culture of their sites. This is a failure which must be corrected in the next 25 years, especially if we are to obtain a reliable picture of the levels of cultural development in the sites.

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AN OLD WORLD VIEW OF NEW WORLD PREHISTORY *

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ABSTRACT

A review of problems in American archaeology is organized around the Lithic, Archaic, Formative, Classic, and Postclassic stages of Willey and Phillips. Among the problems raised and discussed are: the inadequacy of the Early Lithic or generalized gathering tool stage, the need for a better definition of the Archaic concept, the difficulty in extending the Archaic stage into South America, the dating and interpretation of the Peruvian Preceramic period, the origin of New World cotton, the possibility of trans-Pacific contacts and the feasibility of ocean travel, the Mesoamerican origin of the Formative stage, the dating of Valdivia culture and the Formative in general in South America, the relationship between Chavín and the comparable cultures in south coastal Peru, the inappropriateness of identifying most of the Mesoamerican centers as cities, the Aztec state as an empire, and the Classic stage as essentially urban, and the possibility of yet finding a Siberian origin for the pressure-flaked bifacial projectile points of the paleo-Indians.

I VERY MUCH appreciate the invitation to come to the United States and address you at this time, although perhaps I do not feel as much of an outsider as I ought to be in order to do full justice to what I am expected to do. Those archaeologists in Europe who are interested enough to try and keep up with the main lines of American research are a small body, and somewhat isolated from one another. Those of us who live in Great Britain are a pathetically small handful, and until recently no attempt was made to teach the subject except at Cambridge, where I give a short course which is an optional extra to a few students, and at Oxford where Miss Blackwood used to give an even shorter one. Now, with her retirement, we have come down to one! The result is that I constantly find myself looking across the Atlantic rather than the English Channel, although I am painfully conscious that I do not see enough of what is going on there. However the fog of European isolation has some chinks in it, at any rate in Cambridge. Grahame Clark's term at Harvard was all to the good; Glyn Daniel is very ready to spread the knowledge of the more important New World developments in the magazine *Antiquity*; and another colleague of mine, Charles McBurney, whom I hold to be one of the best authorities on the Paleolithic, was persuaded to stick his neck out with me on the subject of American origins (Bushnell and McBurney 1959). Perhaps he stuck it further out than he otherwise might have done because he was born in Massachusetts, although it would be hard to detect any signs of it now! Finally, it is good to have my *compadre* Eric Thompson living near us; he has brought something of America home with him besides Maya glyphs because he has rechristened his house Harvard. I am glad to say that his knowledge and the value of his branch of study have been recognized by his election to the British Academy, a distinction reserved for our foremost scholars.

I take it that what you expect me to do is to look at New World archaeology as a whole and pick out what appear to me to be the outstanding problems, and other matters of interest. A good deal of what I have to say may not be new to you, but I hope that some of it will serve a useful purpose in directing attention to matters which need further investigation or perhaps a fresh approach.

A thing of which I have constantly to remind myself is the vast areas you have to deal with, and when it is realized that England and Scotland fit comfortably within nine degrees of latitude it becomes easier to realise what problems you face for example in the Great Basin and Oregon, and what great discoveries must still await us. It means that in the United States at least you all have to be pioneers still, except in part of the Southwest, and it is greatly to your credit that we have an outline of the picture. The state of affairs in England is very different. There is of course plently to be learned in detail, but the information accumulated in the last century or so has given us from all sources (including historical and geographical ones) a knowledge of the country as a whole, which I could perhaps compare with the intensive picture of the Virú Valley which your researches have provided. I will add in passing that we have to realize also the vast size of Siberia, and I want to emphasize the possibility of future discoveries there which may help to settle the argument about American origins in which I have been rash enough to involve myself. I do not intend to follow that particular

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line any further at this point, but I hope to return to it at the end of my lecture, since it seems to me to be a matter in which cooperation between the hemispheres is peculiarly necessary.

My next point is a matter of communication. I am not one of those who think it a delusion to imagine that we speak the same language, and I hope that I am in less danger of misunderstanding you than I am, for example, some of my countrymen from Yorkshire. Nevertheless differences in terminology may creep in and they may lead to difficulty. I experienced recently an instance of minor importance in connection which pottery, and others will come up in the course of this talk. The cause lies in the small numbers of those in either field who have time to read the literature of the other half. In the nature of things most of us read little outside our own province, except that which deals with general principles. I believe, to judge from the references in your papers, that a good many of you read Childe and some read Clark, but I have not noticed a great rush in England to read Willey and Phillips (1958), although I feel that perusal of their Part I might at least relieve the unfortunate word "culture " of some of the load it has to carry.

The mention of this book, *Method and Theory in American Archaeology*, leads me to make the obvious remark that it must give much food for thought to anyone who is trying to study this hemisphere. I am not here to review this book, but it has stimulated my thought to such an extent that I cannot help using the scheme which the authors employed and extended in their Historical-Developmental approach as a sort of thread on which to hang much that I have to say. Anyone who tries to teach about New World archaeology will agree with me that much of what they say expresses our own thoughts. With some of it we may disagree, but the amount of reference I make to the work, whether to agree or not, is a measure of my debt to Willey and Phillips.

From a European angle, the first thing that struck me was the amount of space they found it necessary to devote to explaining that cultural stages were not necessarily chronological periods. (I say "necessarily" because in some cases the time factor can virtually be ignored. It depends partly on the size of the area studied, and partly on the degree of detail you are going into.) They showed good reason for this, but it seems a truism to those who are used to the idea that there were advanced civilizations in the Near East while the British Isles were still enveloped in a late Mesolithic fog, or, on a more intimate scale, that the Perpendicular style of Gothic architecture was well developed at Gloucester when one of the greatest monuments of its predecessor was still being built at Ely, not so very far away.

Passing on to the main sequence of Lithic, Archaic, Formative, Classic and Postclassic stages, I must say at the outset that I am impressed with the difficulty of applying the whole of any such scheme to the whole American continent. The authors recognize this to some extent by confining the Classic stage, rightly in my opinion, to Middle America and the Central Andes. In company with them, and doubtless with many others, I have for several years found it convenient to extend the use of the terms Formative, Classic and Postclassic from Middle America to Peru, while recognizing important differences between the two areas in the two latter stages. I think I originally did this largely because Wendell Bennett's terms, Cultist and so on, are not very euphonious to our ears, although they have the very great merit of expressing exactly what he meant to say. I also confess to a prejudice, I don't think it is more than that, against the word Florescent, which a misprint on the dust cover of a recent book from Columbia University Press (Strong and Evans 1952) did nothing to dispel:

> Cultural Stratigraphy in the Virú Valley, Northern Peru
>
> The Formative and Fluorescent Epochs
>
> By William Duncan Strong and Clifford Evans

Somehow I think that "fluorescence" is a little pale for either of the authors; if they emit anything I should call it a rosy glow!

To pass to matters of more substance, how right I feel Willey and Phillips to be to drop Early Lithic, although I find Willey wavering back towards it in a more recent publication (Willey 1960). I find the generalized haphazard forms of stone implement, which turn up at all sorts of levels, profoundly unsatisfactory as chronological tools, but the idea that they have to be very old dies hard. On the general question, I will risk my neck by expressing the opinion that all such finds which have so far been claimed as Early Lithic are shaky, and that the mere multiplication of shaky instances does

not increase their probability. It would be a different matter if there were a single satisfactory example on which to base them.

In studying the colonization of South America, I think it would be useful if we could get some help from the geographers in making general maps of the distribution of the tropical forests at various stages since, say 10,000 B.C. and similarly the later stages of the distribution of the ice sheets. We are probably a long way from having enough information about the forests, but there may be more data on the ice. Evidence is accumulating that occupation by man was pretty widespread by about 6000 B.C.; apart from the fact that man may have been in Patagonia earlier than we have been thinking, and the date of about 6000 B.C. for Ayampitín. Laming and Emperaire (1958) claim to have a date of the same order from a Brazilian *sambaqui*.

I must now pass on to the Archaic, a most difficult matter. In reading *Method and Theory* I could, as it were feel the travail of the authors' souls in their efforts to produce a definition which would catch everything in that aching void between Lithic and Formative. Their brief definition of it as "the stage of migratory hunting and gathering cultures continuing into environmental conditions approximating those of the present" is fine as far as it goes, but it combines a wide variety of cultures (I quote that bad word from the book itself) even in the Eastern U.S. Thus, some have a well-developed pottery technology, and some even come near to transgressing the definition itself by not being altogther migratory. I quote: "Settlements (*sic*) are characteristically small in extent, but the depth of deposit often indicates considerable continuity, from which a sedentary or at least seasonal type of occupance can be inferred." I am inclined to believe that it would help to give meaning to the definition by adding to it the old one, whose source I forget, which defines Archaic sites by the things that they lack, especially earthworks and substantial dwellings. I wrote this before reading Fowler (1959) or Caldwell (1958); it may well be that the idea of defining the Archaic in terms of successful adaptation to various types of environment is a workable one.

I do not want to labor the point as regards the United States (it is hard enough for those who work there), but I feel even greater difficulties about extending the concept to South America. However difficult it may be in detail, I would think that the Archaic in North America had a continuous distribution in time and space, and that most of its manifestations were related, however remotely to one another. If I understand Caldwell (1958) rightly, this is certainly true of the East which he calls a diffusion sphere. Willey and Phillips (1958) say that the term "historical-developmental interpretation expresses the fact that our stage concepts are not wholly abstracted from the historical matrix," so they would presumably concede that this point has some importance. With the possible exception of the earlier *sambaquís*, I am doubtful that the examples they cite from South America have any sort of historical connection with those of North America; I do not suggest that this excludes the possibility that some of them are on a roughly Archaic level, but I think that it does impose the necessity of tightening the definition up if it is to have much meaning. Above all, I am troubled about the Preceramic of the Peruvian Coast, which is described as a possibility for an Archaic Stage; owing to the activities of Engel (1958), we know a great deal more about it than we did when *Method and Theory* was written, and I do not know what the authors now think. Whatever its relationships may be, it is surely quite isolated from the Archaic of the United States. I would not call it Formative, not only because of what it lacks, but because there seems to be a definite break between it and the recognized Formative which starts with Chavín, and has, I was going to say, its roots in Middle America, but perhaps that is rash! On the other hand, the Preceramic does have substantial dwellings and perhaps ceremonial structures, and I maintain that to stretch the Archaic to cover it is to divest the term of any meaning at all. Perhaps we had better recognize that the Archaic is not necessarily a Pan-American stage, any more than the Mesolithic is world-wide. (I make this remark because Engel has used Mesolithic also for the Peruvian Preceramic, and was so convinced that the two were comparable that he started a correspondence with Grahame Clark. If you want a New World equivalent of Clark's famous Star Carr site, look for it at The Dalles, Oregon, but don't ask me to support you if you call it Mesolithic!) I would advocate calling the Peruvian Preceramic by its own simple descriptive name, until we know enough about

its origins and relationships elsewhere to find a better.

While I am on this subject, I would call attention to the fact that the origin of this Preceramic is one of the most burning questions with which we are faced, and the fact that Engel has followed up Junius Bird's great discovery and given us over 30 sites scattered over most of the Peruvian coastline has greatly increased its urgency. The Peruvian Preceramic has a remarkable art style featuring, among other things, double-headed serpents, fish, and other creatures, found chiefly on textiles, but also engraved on bones, gourds, stone, and shells. Would that we had more illustrations of it. It does not look like the first glimmerings of art to me, but rather something very sophisticated, even though much of it be executed in the twined textile technique which is an awkward mode of expression. The whole situation of this Preceramic, appearing full-blown in deposits lying on bedrock at numerous sites, with little development for a long time thereafter, points to a migration from somewhere as yet unknown. The predominance of twining in the textiles of this horizon is itself a remarkable fact, and I wish we could learn more about the distribution of the technique in antiquity. You will recall that Jennings (1957) has suggested that twining as applied to basketry is a New World invention, and in this form it has maintained a strong tradition in western North America. In Europe, its earliest known appearance is Late Mesolithic, I would guess not earlier than the 4th millennium B.C. I only know two areas where it is used for *textiles* in modern times, the Northwest Coast and New Zealand, and it is a reasonable supposition that in both cases it was derived from baskets. Did it arrive in Peru in the form of baskets and was there applied to cotton and bast? If we want to study its total distribution, conditions of preservation are against us in much of our area, but we can at least make sure that we know what we are looking for. Do you teach your students to recognize textile techniques both in actual examples and in pottery impressions? We have a good deal of room for improvement in this respect; students do much more practical laboratory work than they did, particularly in handling stone implements, animal bones and potsherds, but they could pay more attention to baskets and textiles with advantage.

The Peruvian Preceramic raises another awkward question which I think that we hesitate to face, namely the origin of New World cultivated cotton. When the botanists Hutchinson, Silow and Stephens (1947) said that it contained, to put it crudely, 13 chromosomes derived from Asia, and that the only way they could have got to America was by being carried by men in boats, and when cultivated cotton turned up in the Huaca Prieta by 2000 B.C. or earlier, I was inclined to brush it aside as a ridiculously early date for such voyages, and to ask whether the question of natural Tertiary distribution via the Bering Strait region when the climate was warmer (and there was a Tertiary land bridge in spite of this) had been seriously considered. Then to my great pleasure, Hutchinson turned up in Cambridge as Professor of Agriculture and we quickly got in touch; he has assured me that Tertiary distribution will not account for the facts. He is now working on the genetics of American beans, another of the cultivated plants of the Preceramic. The idea of trans-Pacific voyages at this date does not come easily to me, but boats of sorts had been known in northern Europe for at least 4000 years, so there had been plenty of time to develop ocean-going vessels. There will be a more solid basis for such voyages if we ever get dates of the right order for places as near Asia as New Guinea and the Philippines.

If I may wander still farther, I feel that I must say at least a few words on what seems to me to be a rational standpoint on trans-Pacific voyages in general. The Bering Strait land bridge was broken for the last time between 8000 and 9000 B.C. (Hopkins 1959), so any later contacts must have been by boat. There is nothing physically impossible about vessels coasting round the North Pacific at any time after that, and by the first millennium B.C., if not sooner, I would have expected them to be capable of crossing it more directly. In spite of this, I would agree with Rouse (1958) that intrusive site-units are absent from the areas likely to be affected by such voyages (unless we find to our astonishment that the Peruvian Preceramic came across the ocean—it lacks, as Engel notes, any signs of boats), so I think that large-scale movements are ruled out. As to intrusive trait-units, a great deal of subjectivity affects the matter, but to my mind the evidence is so unsatisfactory that it would

seem, in view of the physical possibility of contact, that we have a duty to seek explanations for the differences rather than the similarities. I am inclined to believe that, once the American high civilizations had begun to take shape, they had a kind of sales-resistance to outside influences. Even within the American sphere, it may be necessary to invoke this to account for the extraordinary lag between the development of metal working in Peru and its adoption in Middle America. Doubtless other examples will suggest themselves. I should like to say in this connection how helpful I have found the recent University of Arizona publication on Migrations, but I think I must defend Meggers and Evans (1958) from Rouse's (1958) suggestion that elaborate ceramics were a trait intrusion into the mouth of the Amazon. They have shown that this pottery started to degenerate as soon as it got there, owing presumably to unfavorable conditions, and in these circumstances I cannot see how the trait ever took root at all unless someone brought it, and continued his former practice until he could do so no longer.

The time has come to refer to the Formative stage. Starting with Nuclear America, I confess that I have been accustomed to assume that the Formative of Peru was derived from that of Mexico by way of the west coast, and that Ocós in Guatemala and Valdivia in Ecuador were traces of its passage. I do not need to do more than state that we are concerned here with site intrusions, so migration of some sort needs no justification. The apparent absence of Early Formative sites in Esmeraldas in northern Ecuador and the adjacent areas in Colombia has suggested to me that this was due to a migration by sea, although there is more than a possibility that early sites have been drowned, since these coastal areas show signs of downward warping, in contrast with the rise of the coasts of central and southern Ecuador, and Peru. Some of you have pointed out that Formative sites are generally very close to the coast so they would be very vulnerable to this.

The early dates of from 2400 B.C. onwards which have been published for Valdivia are in contradiction with this theory of Mexican derivation, and would make Valdivia the oldest ceramic site in the New World, but frankly I have my doubts about these dates, and in expressing them I shall raise a general question on the validity of shell radiocarbon dates. Some of my Cambridge colleagues have expressed the view in other contexts that such dates tend to be too early, and that is my impression about them in this region too. There are several shell dates on Inca material which are quite ridiculously too early. Further, there is a gross discrepancy between the Guañape shell dates and those derived from charcoal; in his recent synthesis of New World Prehistory Willey (1960) does not use the shell dates for Guañape although he does use those from Valdivia, presumably in default of any alternative. On the other hand, Evans, Meggers, and Estrada (1959), although they quote both sets, use the shell ones to indicate contemporaneity between Guañape and Valdivia. We cannot have it both ways, and since the shell dates bring us into quite unnecessary chronological difficulties, I see no reason to accept them. For one thing they would make Guañape older than most of the Preceramic, and Bird's dates for the Huaca Prieta have recently been corroborated by a series of six more between 1900 and 1300 B.C. published by Engel (1958) from various sites — he does not specify the material or the method, but he found plenty of vegetable matter, and had no need to use shell. I would regard the honor of living in the cradle of American ceramics as some compensation for 12 years hard labor in an oil camp on the Ecuadorian coast, but I am afraid that I cannot yet lay claim to it! Talk of radiocarbon gives me an excuse to repeat a plea about the datum that I made in a recent article. The use of B.P. dates is objectionable on many counts, of which the chief is that all our historical writings and most of our archaeological thought are in terms of the Christian chronology. As I have shown elsewhere, the most reputable people have been known to make mistakes in addition and subtraction in the process of conversion to this chronology, and the use of 1950 as a datum which was recently proposed (and I hope no more than that) merely introduces another potential source of error. It gets rid of a floating datum, an advantage more apparent than real in the face of the present range of error inherent in the method, but it introduces a datum which will become progressively more meaningless as time goes on.

To return to Peru, I take it that the introduction of cat worship, perhaps maize (does anyone want to derive this from the Ecuadorian coast?), and the stimulus which gave rise to

Cupisnique ceramics, must be due in any case to a slightly later migration (perhaps of a few leaders) than that which brought the first pottery, but the lack of traces of a feline cult in the intermediate area between Mexico and Peru requires explanation. Of course traces may yet appear, but in the meantime I suggest that such people could have sailed straight to Peru sometime between January and March, when the *niño* current sets strongly southward along the coast as far as northern Peru.

I have already taken up a good deal of time on Central Andean Formative problems, but I must pose one more. When I am asked, as I often am, to say a good deal about Peruvian archaeology in a very short time, I generally organize most of what I have to say about the Formative and Classic of the coast under the headings of a North Coast Tradition and a South Coast Tradition. Many of us assume that the North Coast Tradition, starting with Chavín Cupisnique, stems as I have said in Mexico, but what of all that we associate with Paracas and Nazca? I know that attention has been called to the Chavinoid elements in Paracas, and Engel has found a Chavinoid stage in the Paracas region though he has as yet published few details, but surely the Chavín elements in Paracas are expressed very differently from those in the type area, and play but a small part in comparison with the fantastic, otherworldly demons, the strange modeling, the early development of polychrome, the exaggerated deformation of the skull, the trephining, the full clothing, the crouched bundle-burials, and the exaggeration of the cult of the dead as shown by the time and effort put into the textiles. This seems to me to point to stimuli from a source as yet unknown, it is an important problem, and I commend it to your notice.

Turning for a moment to Middle America, I have been much impressed by the support which the recent report on La Venta gives to the development of notable structures, supported by a good deal of labor, in the later Formative (Drucker, Heizer, and Squier 1959). The authors imply that it was a remote sanctuary; does its remoteness point to something like the pilgrimage pattern which Bennett suggested for the contemporary Chavín developments in Peru? This explanation underlines the objection which I feel to the term "Urban Formative"; I tried to voice this in a discussion in Costa Rica the year before last and no one took any notice, but now I am in the pulpit! Of course the builders of these sites disposed of large labor forces in some way, but they did not live in a city. I do not see how you can get round that by calling it a dispersed city, it is changing the plain meaning of words, which is a barrier to communication. (Webster's dictionary says "a town or collective body of inhabitants incorporated and governed by a Mayor and aldermen," also in the New States used loosely!) Since the compulsion involved was by all the signs religious, rather than political or economic, Hieratic Formative would meet the case better, though it is not as euphonious as I should like. Perhaps someone can think of something better — Temple Formative?

When we come to North America, I cannot speak at anything like first hand, but I am impressed by the difficulty of applying the term Formative to areas outside direct Middle American influence, which I understand to be practically limited to the extent of the Mississippian tradition. When Willey and Phillips confess themselves "semantically embarrassed" and describe Hopewell as a kind of florescent northern Archaic culture, I begin to think that the attempt to apply Nuclear American terminology to the Northeast raises more difficulties than it solves.

This is perhaps a good time to pay tribute to Martin, Quimby and Collier (1947), and to say what a blessing it would be to foreigners like myself if they could bring themselves to produce a new edition. To judge from remarks from within the United States, which I have read, about the difficulty of organizing the great masses of data, the task may now be almost impossible, but the very difficulty is a measure of its value. While I am on the subject of books, I want to ask a question about Gladwin's *History of the Ancient Southwest* (1957). Two Americans with whom I have discussed it have reacted differently. One, if I understood him rightly, said that we should be grateful to Gladwin for pulling the whole thing together into a connected story, but that every specialist would disagree with his own section because it was so much easier to quote references from memory than to look them up. The other, less mature, said that he believed that it was on the list of prohibited books. If I may suggest a reason for this, Gladwin's hypothesis of a "red" or communist invasion from Mexico seems as good as

any other! Seriously, the part which impressed me was the story of the gradual abandonment of large areas under pressure from invading nomads, and I shall appreciate any comments about the soundness of this from those qualified to judge. I trouble you with this because summary accounts of this sort, if they can be used, are of incalculable value to outsiders who cannot immerse themselves in a particular field.

After this digression, there is little that I want to say about the Classic stage. The idea that it is *essentially* urban stirs in me the same emotions as the use of the word in connection with the Formative. I can see an urban agglomeration in Gallinazo; from Strong's (1957) preliminary account, I am not so sure about Nazca, which looks more like a large ceremonial center without a concentration of dwellings; Teotihuacán begins to look like a true city; but to call the Maya centers cities seems to me to stretch the term beyond the permissible limits. We all realize the economic dependence of these centers on the peasants scattered around on their milpas. Are those who call them cities emphasizing the economic aspect too much, and could this be a corrupt following of my friend and colleague Grahame Clark? I am inclined to attribute the urban manifestations of the Classic to special circumstances, namely exceptional fertility of the soil generally connected with irrigation; and to irrigation I would ascribe also the militaristic nature of the Classic of the north Peruvian coast. We need to know more about the Classic settlement and irrigation patterns in the south coast before we can speak with any certainty about either feature there. As for Teotihuacán, we have hints of the possibility of irrigation, but no more.

On the Postclassic I want to offer two observations. One concerns the oft-repeated description of the Aztec state as an empire. I have long been of opinion that it was nothing of the sort, whatever it might have become, and in this respect it contrasted markedly with the Inca state which really was an empire. For instance, Soustelle (1955), although he constantly refers to an Aztec Empire, supports me when he comes to define it. He says "Each city preserved its administrative and political autonomy, under obligation only to send tribute, furnish troops, and in the last resort to submit lawsuits to the tribunals of Mexico and Texcoco. There was no real centralization; what we call the Aztec Empire was rather a loose confederation of city-states with very varied laws." The other observation concerns the art of Mexico and Peru. I do not know how generally the contrast between them is realized and expressed, but I have been more and more impressed by the depths to which Peruvian art descends in Postclassic times, whereas that of Central Mexico is full of life and vigor to the end, even though it is pre-occupied with death.

I will end by returning to the beginning. The question of American origins is vital to our studies, and there is a fundamental difference of opinion about the origin of that most prominent feature, the bifacial pressure-flaked point. Two points of view, doubtless well known to you all, have recently been expressed in our journal *Antiquity* and they are briefly these. Chester Chard (1959) said that "difficult as it may seem" pressure-flaking in general and these points in particular developed in the New World independently from crude chopping-tool cultures, on the ground that Asia offered no antecedents of adequate age for them. Charles McBurney and I found it even more difficult to believe that these highly specialized projectile heads could arise from such an astonishingly conservative and impoverished tradition, so we followed up Chard's article by suggesting that bifacially worked projectile heads of Palaeolithic age had already been found as far east as neighborhood of Lake Baikal, and that the accepted late dates for some Siberian sites were insufficiently founded (Bushnell and McBurney 1959). We also gave an indication of the complex ancestry of pressure-flaked projectile heads in Central and Eastern Europe. All I want to do here is to emphasize this, as well as to say that pressure-flaking itself, which seems to have arisen independently in Europe, Central Africa, and perhaps North Africa, has in each case a long ancestry of controlled percussion flaking.

Another point which we made after discussion with Marie Wormington is the overwhelming majority in the New World of butchering sites as against camp sites, in contrast with the Old World finds; this has been noticed also by Haury (1958). Projectile points are so prominent among American finds that we in Europe do not know enough about the other types which may be found with them; some have been published, there are probably many unpublished or at any rate unknown to us, and some of them may give important clues to relationships between the two hemispheres. Again

it is a matter of communications. May I suggest that it would be most valuable if more direct discussions could take place between Palaeolithic specialists on both sides (I do not number myself among them), and particularly if they could examine your collections together.

Now I cannot do better than end by wishing you all as fruitful a second 25 years as the first has been.

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ARCHAEOLOGY AND LANGUAGE IN WESTERN NORTH AMERICA*

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ABSTRACT

The following hypothesis is proposed: that the distribution in time and space of Desert culture(s) and Hokaltecan languages imply a connection, that at one time there was a continuous band of Hokaltecan people practicing Desert culture from the Great Basin to the Texas and Tamaulipecan coasts, that this continuity was disrupted by an incursion of Utaztecan highlanders moving along the cordillera into Mexico, that proto-Shoshoneans entered the Great Basin from the northeast, that the Yuman peoples are a remnant block and not a disrupting wedge of Mexican origin, and that some of the similarities between cultures in California, the American Southwest, and Mexico are survivals of the basic proto-Utaztecan or Macro-Penutian culture, while still others are the result of interchange through a relatively homogeneous cultural medium made up of Utaztecan highlanders living along the western cordillera from central Mexico to the American Southwest.

THE LINKING of archaeological assemblages and linguistic groupings is a risky business. I realize that the practice is often, and sometimes very wisely, looked upon with high skepticism. Nevertheless, I am going to attempt such a linkage, if for no other reason than to pose some problems and to offer some enticing leads for further thought and investigation. I wish to be taken seriously, but not to the extent that I shall be held too closely accountable for what the late President Roosevelt used to call the "iffy" nature of the documentation. Thus, I would like to present a working hypothesis which has just enough support to intimate its possibility and more than enough significance, if true, to make it worth both wider and deeper investigation.

In the first place, the validity of the Hokaltecan linguistic grouping (Hokan-Coahuiltecan) has been established with satisfying assurance (Kroeber 1925: 282; Swadesh 1954b: 641; Greenberg 1953: 283; Greenberg and Swadesh 1953: 221; Bright 1955, 1956; for contrary opinion, see Newman 1954: 632). As to its antiquity, one look at the linguistic map (Fig. 1) will demonstrate the significantly marginal position of people speaking this tongue. This I take to be a sign of age. Furthermore, both Klimek (1935: 61) and Kroeber (1935: 7) expressed the opinion that the Hokan culture-stratum in California was the most ancient in that area, and Voegelin (1945) once said that both by diversity and discontinuity Hokan-Siouan is the oldest language in North America. Although Voegelin (1958) has since offered an alternative proposition, namely that Macro-Penutian is the oldest, I believe that non-linguistic evidence, such as will be presented below, tends to favor his earlier position. In any event, Hokaltecan is early, if not the very earliest, and this dating will satisfy the requirements of my hypothesis.

Turning now to the Desert culture, I should like to call attention to the fact that Jennings and Norbeck (1955: 3), in their original paper, stated that this culture "was not restricted to the Great Basin but was the pattern found, following the disappearance of large game, throughout a wide area of the west surrounding the Basin," and they specifically name as examples the Cochise, Early Basketmaker, and Birdshead Cave cultures. Both Kelley and I have made the same point in regard to the cave materials of west Texas and Coahuila, Mexico (Kelley 1959: 283; Taylor 1956: 219). And Kelley (1959: 288) has shown that cultural assemblages in central and coastal Texas are transitional between the Desert culture and the eastern Archaic, having significant similarities to each. Kroeber (1920: 168), Lowie (1923: 156), and more recently Heizer (1951: 8) have expressed the opinion that the archaeological cultures of north-central California have strong and basic connections with Great Basin (read *Desert*) cultures. Kroeber (1935: 7) explicitly states that the ancient Hokan culture-stratum had Basin (read *Desert culture*) affiliations. Lowie (1923: 156) said that this common culture was as near as possible to the "primeval North American culture." The finds at Borax Lake in northern California, the fluted points aside, display strong Desert culture affinity (Harrington 1948). Wallace has stated (1954: 121) that the early milling stone cultures of southern California have possible linkages with Cochise (a Desert culture) and the Borax Lake culture.

Finally, turning to ethnological data, we find many authorities, among them Spier (1929: 217;

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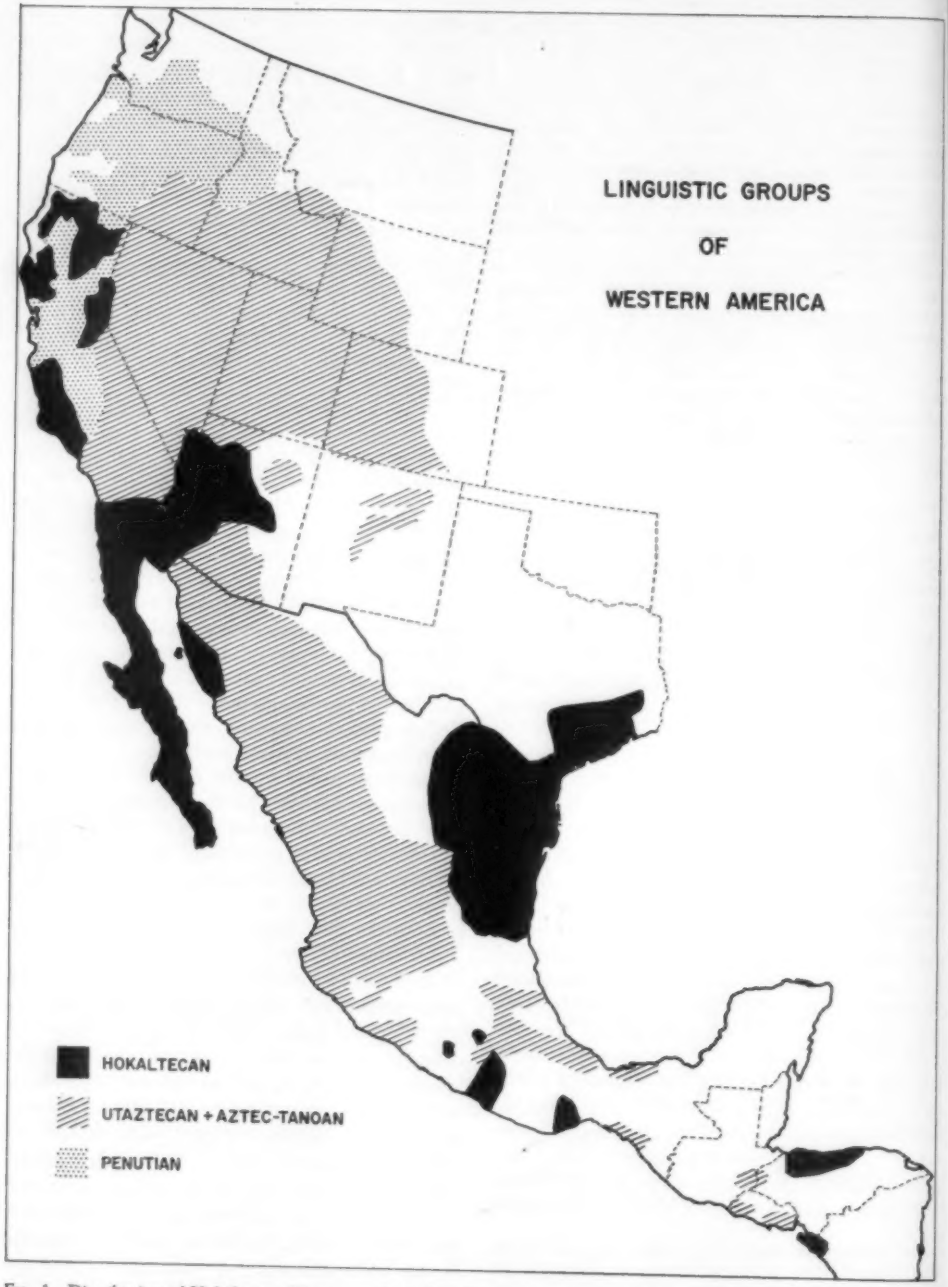

Fig. 1. Distribution of Hokaltecan, Utaztecan (including Tanoan), and Penutian languages in western North America.

1936: 13) and Kroeber (1928: 396; 1939: 45, 50; 1954: 557), pointing to the strong Basin affinities of the Yuman-speaking peoples of the Southwest, southern California, and Baja California. This is particularly so of the Upland Yumans, and I would say that it is also true of the River Yumans if we take from them the obviously late borrowings such as pottery and agriculture, which Kroeber once said lie so lightly upon the Mohave. In this regard, Schroeder (1952: 49) has recently shown that agriculture was acquired by the Mohave only as late as A.D. 900. Finally, if we would analyze the culture of the Seri and of the peninsular Californian Yumans, about whom we know so little, I think it highly probable that their Basin, that is Desert culture, affinities would be very apparent.

Now, when we come to join these bodies of data, the distribution of Hokaltecan languages and of the cultures of Desert culture affinities, we discover a remarkable concordance both in time and in space. Both are early in their areas of occurrence, and these areas of occurrence coincide: in northern, southern, and peninsular California, in the American Southwest, in northwest and northeast Mexico.

The recognition of this coincidence of Desert culture(s) and Hokaltecan speech came as the result of working with archaeological collections from the State of Coahuila, Mexico (Taylor 1937, 1956). From the earliest Coahuila cave materials, with radiocarbon dates of more than 7000 B.C., to the latest, with dates around A.D. 1000 or slightly later, there is a cultural continuum, changing to be sure but essentially a single cultural tradition (Taylor 1956: 231). Still later, about A.D. 1600, when the Spanish arrived and described the Coahuiltecan-speaking natives of Coahuila and northeast Mexico (León 1909), the culture was virtually identical with that of the ancient continuum from the caves. Since no language other than Coahuiltecan is known to have been spoken in this region (except for the obviously recent Apachean Toboso) and since there is a demonstrable cultural continuity, it is reasonable to infer that those people of the Desert culture spoke Coahuiltecan (or proto-Coahuiltecan) from the beginning. Furthermore, when we remember the cultural and somatological similarities between the peoples of coastal and central Texas, Coahuila, west Texas, and northwestern Mexico including Baja California (Woodbury and Woodbury 1935: 43; Neumann 1952: 15–17; Studley 1884; Hooton 1933), the distribution of Hokaltecan speakers over the very same vast region, from the Karankawa to the Diegueño and Pericú, takes on added and, I suggest, significant meaning. In this regard and in passing, I should like to point out that the two measurable crania of the earliest three from Ventana Cave, coming from deposits identified as Cochise, are said by Gable (1950: 513–4) to have their closet resemblance

> in a series of undeformed hyperdolichocephals from the Texas Coast described by the Woodburys, and these latter crania, according to the same report, compare rather closely with other Texas crania defined by Hooton and Oetteking. The Woodburys also point out the physical resemblance of this long-headed type with the Pericue of Lower California who are "by some considered an archaic physical variety of American Indian."

When taken in connection with my present hypothesis, this statement would seem to emphasize the Desert culture affiliations of these skulls, rather than the tenuous similarities with modern Papago mentioned by Gable.

There remains, however, one area in which the Desert culture has been found but in which vast area today there is only one Hokaltecan-speaking group. I refer, of course, to the Great Basin itself. This region of Utaztecan speech must be explained or else my hypothesis is mortally weakened. I believe that it can be explained, at least to the extent of suggesting a working hypothesis, by reasoning from two, more or less basic and accepted anthropological premises: the doctrine of limited possibilities (Goldenweiser 1933; Rands and Riley 1958) and the age-area concept (Wissler 1923: 57; 1926).

In areas of stringently marginal subsistence where cultural responses are strongly influenced and restrictively channeled by the limiting effects of the environment (Steward 1938: 261), it seems to be a fact that one mode of life, variable but basically a single adaptation, tends to develop and to spread to the limits of the region. As Goldenweiser (1933: 45) said, "wherever a wider range of variability in origins and developments coexists with a limitation of end results, there will be a reduction in variability, decrease in dissimilarity, and increase in similarity or convergence." When we add to this the integration of borrowed culture traits (Linton 1936: 347–66), it seems probable that the

more limiting the environment, the more convergence and parallel development can be expected in cultures occupying that environment. Succinctly, this means that a single, generic cultural adaptation with only limited variations will develop and spread.

I suggest, therefore, that a prospering (if not a prosperous) adjustment in ancient times to life in arid western North America would have tended to diffuse over the entire region and that any society coming into the area would have converged toward a not-too-different variety of the basic adaptation. This convergence would be particularly noticeable in the realm of subsistence and artifactual traits, that is, in those traits most commonly recovered by archaeological investigations. Thus, it is possible to suggest that whichever of the two groups made the earliest adaptation to life in arid America, both Hokaltecans and Utaztecans would eventually have arrived at cultures which were generically much alike, particularly in those traits by which archaeological cultures are usually compared. I am optimistic that detailed and intensive analysis might differentiate between variants of the Desert culture (Taylor 1943: 309–10) and be able to settle the question of relative priority among them. But such a study has not been made and is no part of the present paper.

However, I do believe that a hint of this relative priority is present in the available data. This is where the age-area concept enters. In western North America, it is plain that Utaztecans occupy a central position with respect to Hokaltecans (Fig. 1). Also in the microcosm of California, the Penutians, said to be distant linguistic relatives of the Utaztecans (Whorf 1935: 608; Greenberg 1953: 283), are surrounded by Hokan speakers. If we assume that the Hokaltecan variety of Desert culture originated somewhere in arid western America and reasonably near the northern limits of that vast region, the marginal distribution of the Hokaltecans becomes all the more striking. Applying the age-area concept to these data, I suggest that it was the Hokaltecans who were the earliest of the two stocks in this region and thus most probably the first of the two to adapt to life in the western deserts. At present, I make no assumptions as to whether or not the Hokaltecans were the originators of the Desert culture or whether the early Utaztecans borrowed directly from them — or from a third party.

From the viewpoint implicit in the facts and inferences brought forward above, it is possible with the aid of some additional data, specifically lexicostatistical data, to construct a working hypothesis, not only for historical events having to do with Desert culture(s) and Hokaltecan speech but also for events pertinent to the culture-history of Utaztecan and Penutian speakers. This hypothesis is as follows:

Some centuries before 10,000 years ago (the earliest radiocarbon dates for Danger Cave; Jennings 1957: 93), Hokaltecan speakers moved from the north and northeast or both into the desert habitat of the northern Great Basin. There they developed or took over what we now call Desert culture. Once adapted to this arid region, the people began to expand. There were two lines of this expansion, an early one heading southward following the desert and a later one westward over the passes of northern California. The southward, strictly desert expansion carried through the Basin, the American Southwest, and into northern Mexico, where some groups followed the arid environment eastward, finally to occupy the desert lands all the way to the delta of the Rio Grande.

At this point, a chronological problem arises. The cultural continuum in Coahuila, which I infer pertains to Coahuiltecan speakers, has produced radiocarbon ages in excess of 9000 years. On the other hand, Swadesh (1954a: 362) has calculated that the separation of Hokan Washo from Coahuiltecan Comecrudo, two languages at the geographical extremes of the Hokaltecan range, is of the nature of only 55 minimum centuries. These figures, if true, require explanation. One solution would be to assume that by 9000 years ago there was a continuous band of Hokaltecan speaking peoples from the northern Great Basin to northeastern Mexico and that the breaking of this linguistic continuity began to occur only as late as about 5500 years ago. There is evidence which seems to suggest this possibility.

In the first place, Swadesh (1959b: 330) says that "from a study of linguistic maps, it would seem that hunting-gathering groups, when not too much impeded by terrain, may long maintain a relatively uniform type of speech over a large area despite a low concentration of population and slow means of travel." Thus on

theoretical grounds, the geographical distance between the Basin and northeast Mexico seems to offer no real problem. It may be significant that, in the cave culture of central and northern Coahuila, the material aspect of culture endured virtually without change for a least 10,000 years. This may be indicative for other aspects of culture, language among them.

In the second place, chronological data are permissive: Swadesh (1959a) gives the internal time-depth of Macro-Yuman (in which he includes Coahuiltecan and Seriyuman) as 92 minimum centuries. This dating gets us well back within the time required to accomodate the radiocarbon dates for Coahuila. Further evidence, suggesting the cause of the fragmentation and suggesting an approximate date for it, will be presented below in discussing the movements of Utaztecan speakers.

Thus according to the hypothesis, by a date which is possibly nearer 10,000 than 9000 years ago, Hokaltecan speakers were adapted to an arid environment and occupying desert territory from the northern Basin to northeastern Mexico (for similar opinion, see Sauer 1954: 555). At some time after this date and before 5000 years ago, certain Hokan groups began to move westward out of the Great Basin and into California over the northern passes. These were the people who brought the Basin-like Hokan culture-stratum, the earliest in California. They spread southward, occupying California at least as far south as the historic Chumash and probably into Baja California as well, where they were bottled up and pushed to the very tip by later peoples, specifically the Yumans (Massey 1947, 1949; Rogers 1945).

At some time after the Hokan-speakers had established themselves in California, Penutian speakers entered the region. It is probable that they came from what is now Oregon, which Swadesh (1956: 41) says "may represent the center of the ancient Penutioid area." In a later paper, Swadesh (1959a: 10) gives the maximum internal divergences for both Penutian and Macro-Nawan (including Utaztecan) as 100 centuries. If the concept of Macro-Penutian is valid, the split between Penutian and Utaztecan was thus probably in the neighborhood of 10,000 years ago and somewhere in the mountainous region north of the Great Basin. In fact, the Macro-Penutian speakers, as they moved southward along the western flanks of the northern Rockies, may have been split by running into the block of Hokatecans already occupying the desert habitat of the northern Basin. In any event, some of the Penutians were diverted toward the west into Oregon and eventually California, while other Penutians and their linguistic relatives the Utaztecans continued down the cordillera. Since the divergence between Yokuts and Chinook is said to be about 55 centuries and the separation of Washo and Yana 50 minimum centuries (Kroeber 1955: 95–6), the entrance of Penutians into California and the consequent breaking of the Basin-California Hokan-speaking continuity may have been at this time. That "explosive disruption" which Kroeber (1955: 102) noted among the California Hokan speakers between 35 and 40 centuries ago could have been one of the later effects of this incursion which involved the usurpation of the Central Valley by the Penutians, the relegation of the Hokans to the peripheries, and the beginnings of the latter's migration(s) southward. In view of the lexicostatistical dates of 50 centuries for the Washo-Yana split, of 48 centuries for that of Washo-Yuma, and of only 37 for that of Yana-Yuma (Kroeber 1955: 95), it seems probable that the Yuman-speaking peoples were part of this later California movement, rather than that of the earlier, Basin-Mexico expansion. Kroeber (1955: 101) includes Yuman in "Pacific Hokan," setting this group of languages apart from Washo and Coahuiltecan. The fact that "Diegueño is set off against all other Yuman languages" (Winter 1957: 21) may indicate that they were left behind in southern California while other Yumans continued their movement southeastward to diverge at some later time either in southern California or in their ultimate location along the Colorado River. Kroeber (1955: 102) has said that the further diffusion of Pacific Hokan probably occurred at this time and that the "five groups in Pacific Mexico–Honduras–Nicaragua might conceivably be the results of one major migration, of groups which, though already differentiated, shared common features, but dropped off the route one by one on the way."

If the foregoing has been an approximation of actuality, we have the Utaztecan peoples separating from their Macro-Penutian relatives somewhere in the mountainous region north of the Great Basin. While working on an early draft of the present paper, I had come to the conclusion that Utaztecans were originally a

mountain, or at least a highland, people and that they had moved from north to south primarily along the flanks of the western cordillera. It was reassuring, therefore, to find that, independently and on different grounds, Romney (1957) had also identified them as mountain people. I was further reassured when it became apparent that, although many Utaztecan groups are desert dwellers, by far the greater number are highlanders and, more significantly, within virtually every linguistic subgroup of Utaztecan there are both highland and lowland (mountain and desert) peoples: Shoshoni-Panamint, Bannock-Paviotso, Ute-Paiute, Tepehuan-Pima-Papago, Opata-Tarahumar-Cahita-Concho, Cora-Totorame, Acaxee-Tahue, Huichol-Guachichil (Steward 1937; Kroeber 1934). To account for this high-low distribution, I infer that, as the Utaztecans moved southward, they left remnant peoples in the mountains and from time to time sent offshoots into lower elevations and more arid lands. Such a "magic flight" hypothesis, this dropping of linguistic entities along a line of migration, would be the same as that which Kroeber (1955: 102, quoted above) postulated for the Pacific Hokan trek southward, but I might also add that it would not fit very well into the types of linguistic migration defined by Dyen (1956) and simplified by Diebold (1960).

It is also true that my hypothesis is, in some ways at least, difficult to reconcile with the ideas concerning Great Basin linguistic prehistory proposed by Lamb (1958: 99) who says: "Around five thousand years ago, or somewhat later, Proto-Utaztekan was slowly beginning to spread into a number of dialects. It may have been located somewhere in the neighborhood of the boundary between Arizona and Sonora." He further postulates a homeland for the Numic tongues (Plateau Shoshonean) somewhere in the vicinity of modern Tübatulabal and infers a very recent movement of this speech, less than 1000 years ago, northeastward into the Great Basin.

His first date I do not find difficult to believe, although it might be a little late. I take it that he bases it upon the separation of Mono and Mexihca, which Swadesh (1954–55: 180) gives as 47 minimum centuries. On the other hand, his locus of Utaztecan divergence and especially his locus and date of Numic separation all seem to me to "compound the hypothesis" to such a degree as to make them decidedly suspect. His reasons for citing the Arizona-Sonora border appear to be based on Swadesh's statement that "The cradle of Utaztekan could be situated some place between" Kiowa-Tanoan and Chibchan (1954–55: 182; see also 1954c: 324). But even though the mountainous barranca regions of southern Sonora and northern Sinaloa are (were) the center of a considerable Utaztecan dialectical variability (Sauer 1934), it seems to me much too complicated to postulate (1) a separation from proto-Penutian in the dsitant north, (2) a migration southward to northern Mexico, (3) a divergence into many languages, and (4) a reverse migration northward again into the Southwest, the Great Basin, and the very region of their separation from the Penutians some 9000 years before. Perhaps this actually took place, but until more specific evidence is at hand to support it or definitely to belie contrary propositions, it would seem better practice to use as a working hypothesis some other, less complex, less compound hypothesis.

Such an hypothesis is the one which I am developing here. To me, the distribution of languages in Mexico (Fig. 1) strongly indicates that Utaztecan (and probably Penutian before it) should be recognized as an intrusion from the north. The modern distribution certainly indicates this, particularly if we remember that the Utaztecan gap in the Southwest is due to the Apachean intrusion and that, if we should draw the map as of, say, A.D. 1000, there probably would be no gap at all. An examination of the effects of the Utaztecan incursion upon other language communities is beyond the scope of the present paper — except to point out that the distributional data suggest that it either broke a Hokaltecan continuity in northern Mexico or filled a break already made in that continuity. The date may be reflected in the date of the Washo-Comecrudo separation, that is, in the neighborhood of 5500 years ago. It may be corroborative that about 6000 years ago the cave culture of Coahuila, presumed to pertain to Coahuiltecan speakers, showed signs of disturbance (although no break in continuity), specifically (among other evidence) in the disappearance of the Cienegas complex and the marked change in projectile points from a basic two-type inventory (contracting-stem and laurel-leaf) to a wide and seemingly random variety of notched and stemmed forms. There seems to be nothing in Swadesh's (1959a) most recent lexicostatistical data which would make im-

possible such a date for the entrance of Utaztecan speakers into Mexico, especially if we postulate that Penutian speakers preceded them by at least a few centuries.

If we assume this northern intrusion, then it seems to me a much more elegant and probable hypothesis that the internal divergence of Utaztecan occurred, not as far south as the Arizona-Sonora border nor in any one locality, but rather as a divergence which started at a latitude north of the Great Basin and which continued to produce a series of "remnants" and "remnant offshoots" which were left behind as the main body of Utaztecans moved southward along the western cordillera. Of course, it is a virtual certainty that this "main body" would itself have been a composite agglomeration involving a number of already distinct but related linguistic entities and that the separation of remnants was a continuing process covering a considerable temporal and geographic span.

I believe that somewhat similar arguments can be used to support a hypothesis as to the divergence of what have usually been known as the Plateau Shoshonean languages. Lamb (1958: 98) says that, because one language of each of the three pairs of languages in the Numic (Plateau Shoshonean) family is now located in the southwest corner of the Great Basin, in the region of Death Valley, this region is the homeland from which the languages spread by migration northeastward. He also says that this spread must have been very recent because the dialectic differences within the several Numic languages are very slight, particularly so in view of the vast areas that they occupy. On *a priori* grounds, both his points appear to be tenable—although Swadesh (1959b: 33, quoted above) implies that linguistic similarity over large areas does not necessarily mean recency of occupation—until we come to account for the geographic distributions, not only of Numic and other nearby Utaztecan languages but also of the surrounding Hokaltecans. When we do this, it is apparent that only by considerable compounding of the hypothesis can any explanation be given, and then only one which for several reasons seems highly dubious.

If we accept Lamb's assumption that the homeland of Utaztecan divergence was somewhere along the Arizona-Sonora border, we must also make one of two further assumptions: that in reaching their present locations in southern California, the Shoshoneans arrived (1) *before* or (2) *after* the Yumans had reached the Colorado River valley. If the Shoshoneans arrived *after* the Yumans, we must postulate a very circuitous flanking movement from the border to the north, west, and southwest, then a settling in, then a divergence of 40 (Luiseño-Tübatulabal) to 30 (Numic-Tübatulabal) minimum centuries, and finally a reverse movement 2000 years later northeastward into the Basin and on into the northern mountains (dates from Swadesh 1954–55: 176). If we assume that the Utaztecans arrived *before* the Yumans, how then do we account for the split between the southern Hokans and their linguistic relatives the Yumans, a separation which certainly appears to have been caused by Shoshonean intrusion from the northeast (Kroeber 1925: 578, but with doubtful dates)?

If, however, we assume a Utaztecan "homeland" changing with time all the way from north of the Great Basin to somewhere *in* Mexico, these problems are eliminated, as are many others having to do with Utaztecan distribution. To me it seems entirely possible, taking into consideration the theoretical considerations and the substantive data, that sometime before 5000 years ago people speaking Utaztecan began to move southwestward across the Great Basin from locations along the western flanks of the northern Rockies. This incursion may have been responsible for starting the Hokaltecans on their way to California, an event which has already been postulated for this approximate time. This may also have been the time when Shoshonean acquired those instrumental or thematic verb prefixes which puzzled Kroeber (1955: 94):

> If these [instrumental verb] prefixes are confirmed as limited to Basin-Plateau Shoshonean, they must represent either an autonomous structural growth in proto-Plateau Shoshonean or perhaps more likely a development due to stimulus from outside, such as from Siouan. With the approximate age of Plateau Shoshonean determined by glottochronology, we might then be in a position to reconstruct an inference on what division of Siouan (or wider stock) had been the influencer.

I suggest that it was Hokan, of the "wider stock" of Hokan-Siouan. Both Washo and Achomawi-Atsugewi, the Hokan languages nearest present-day Plateau Shoshoneans, are prefixing languages (as is Yuman incidentally; see Halpern 1946: 271), and Dixon and Kroeber (1919: 91) have already proposed that

Maidu, a normally non-prefixing Penutian language, acquired instrumental prefixes from these people. Whether the absorption into Shoshonean was early or late, I have no information. But it does seem possible, even quite probable, that it could have been at a time when both Shoshoneans and Hokaltecans were living in the Great Basin practicing Desert culture(s). Perhaps a study of the prefixes themselves and of the verbs to which they are commonly attached or of both might, after the fashion of reconstructing a proto-language, provide some leads in the direction of solving this problem. As these proto-Shoshoneans moved, their language(s) changed, and so did the speech of their relatives back in the "homeland." I see no *a priori* reason to postulate, because lexicostatistics say that Luiseño separated from Tübatulabal a minimum 40 centuries ago, that this separation had to be in the present locus of either of these languages. Who is to say at present that the divergence did not take place in a third region, some common ground back across many miles and many centuries of trekking? Why is it not possible that the recent divergence within Numic tongues occurred among remnant peoples left behind in the northern and central Basin, while their linguistic near-relatives continued southwestward with their already diverged dialects to end up in the stoppered funnel created by the antecedent Luiseño, the Sierra Nevada on the west, and the already established Colorado River Yumans on the east? The splitting of the Ute-Paiute and Hopi could have happened at the point of contact with the Yumans or at some earlier point (see Hale 1958). I can see no more reasons to propose Death Valley as the homeland for Numic divergence, on the sole grounds of multiplicity of present-day languages (Lamb 1958: 99), than to identify, for example, the British Isles as the homeland of the Celtic languages! We know that Celtic-speaking peoples of the British Isles represent a pile-up at the end of a long migration—why not California Numic?

That is the hypothesis. On the basis of this broad scheme, it is possible to explain certain other linguistic and cultural distributions in western North America. For one thing, it is obvious that the view which has usually been held concerning the Colorado River Yumans should be changed. It has been fashionable for a long time to regard these people as relatively late migrants from Mexico, who thrust a wedge between formerly contiguous Utaztecans (Strong 1927: 53, 57; Underhill 1954: 647, 656; for contrary view similar, except in chronology, to the present one, see Rogers 1945: 168). It seems to me that recent field work, both archaeological and ethnographical (Rogers 1939, Drucker 1941, Haury 1950, Schroeder 1952), has thrown this view in serious doubt. And it is significant, I believe, that Albert Schroeder (1960) has recently advanced the hypothesis that the earliest known dwellers along the lower Colorado and Gila rivers, the people we have been identifying as belonging to the Pioneer Period of the Hohokam sequence, were similar enough to later Yuman groups as to be in all probability their cultural and linguistic ancestors. Furthermore, not a few specific traits link the ancient Hokan culture-stratum of California with both Schroeder's Lower Colorado River Pattern and with the Desert culture(s): earth lodge with four posts, cremation, diagonal twining, one-piece moccasin, conical burden basket, basketry seed beater, stone boiling in basket, hoop and pole or dart (game?), tattooing, and possibly the deer-mask decoy, pounding slab with loose hopper (twined), quiver of animal hide, triple arrow feathering (radial?). This list includes all but one of the archaeologically discernible Hokan-Yuki traits listed by Klimek (1935: 40). I suggest, on the basis of this evidence and the general hypothesis of the present paper, that the Yuman-speaking peoples are a remnant population, not an intrusive wedge. They should be considered the survivors of Hokaltecan peoples who came from farther away in California around the southern tip of the Sierra Nevada. It is also postulated that they were in residence along the Colorado and Gila rivers when the first Utaztecans came in contact with them, from whatever direction this incursion will eventually prove to have been.

For another thing, the present hypothesis permits a new interpretation of the similarities between the California Kuksu cult and the god-impersonations of the Southwest and Mexico. These have been viewed as recent diffusions from the Southwest (Kroeber 1932; Loeb 1932a, 1932b). I believe that it is now possible to look upon them as intensifications (with possible reinforcement through secondary diffusion?) in California, in the Southwest, and in Mexico of traits generic to a very early culture-stratum which pertained to peoples speaking Macro-Penutian languages, that is, to the proto-

Penutian and proto-Utaztecan peoples. I can see no more reason to assume recent diffusion for god-impersonation than for such traits as the earth lodge, sandals and the apron, twined and coiled basketry, girls' puberty rites, and not a few other "basic" traits which are found in the earliest culture-stratum in California, in the Southwest, and in Mexico, and which, to my knowledge, have never been identified as being recently diffused from the Southwest into California.

Finally, I suggest that the vast pool of Utaztecan highlanders that occupied the western cordillera constituted a relatively homogeneous medium through which cultural influences passed back and forth between the high-cultures of Mesoamerica and the American Southwest. Linguistic barriers would have been reduced and probably were even less in the early years, and a series of basically similar cultures, only recently diverged from a common base, would have further facilitated the cultural interchange which current research is clearly demonstrating was both broad and in amazing detail (Ferdon 1955; Jennings 1956; Johnson 1958; Dutton 1959; Kelley 1960). Furthermore, the lowland, desert or riverine exclaves from this pool constitute a widely variable but related series of ecological adaptations by what was probably once a relatively homogeneous basic culture, that of the Utaztecan highlanders who entered Mexico some 5000 to 6000 years ago. From this point of view, they appear to me to offer a fascinating and promising field of anthropological research endeavor.

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MICROLITHIC SITES IN THE ASIATIC USSR

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Abstract

Microlithic cultures of the steppe and desert zone of Asia include the Kel'teminar of the Aral Sea region and comparable cultures in Kazakhstan, Turkmenia, Trans-Baikal, Mongolia and China. This microlithic industry is characterized not by the size of the tools, but by the use of blades rather than flakes for the manufacture of stone tools. The prismatic core, often small and pencil-shaped, is very common; bifacial retouch is rare. The microliths are consistently found in association with pottery and bifacially worked projectile points of the Neolithic and Chalcolithic of the third and second millennia B.C. The tendency of Soviet archaeologists to seek the ethnic affiliations of archaeological complexes may explain the considerable regional variation in pottery and stone tool inventories, but adjustment to environment is a more likely explanation for the wide-spread occurrence of the basic features of the microlithic industry. A way of life based on migratory hunting of small game and collecting developed as a result of refinements in missile weapons which included the bow and arrow. These developments can be correlated with the introduction of the microlithic blade technology. This way of life and the use of microliths survived in the steppe and desert areas, but in the regions to the south where farming developed the microliths were rapidly replaced by heavy tools such as hoes and pestles, and in the northern forests harpoons, fishhooks, and heavy wood-working tools indicate a shift to a more sedentary life based on intensive fishing. The widespread microlithic survivals in northern Asia demonstrate similar subsistence patterns in similar ecological zones rather than basic ethnic unity. Suggestions concerning origins, historical and cultural ties, and possible population movements are gradually emerging from detailed studies of the boundaries between the microlithic zone and the pottery and forest areas as well as from broader comparative analyses.

FOR THE PAST 20 years Soviet archaeologists have concentrated on investigation of local differences in the material culture of the Stone and Bronze ages. These investigators have attempted to approach in this way the problem of the ethnic affiliations of early man. However, in studies of the history of early cultures, the archaeologist must add a reservation to the effect that not all differences in the material culture of ancient peoples are necessarily connected with ethnic differences. Even so, all recent works view local variations in material culture as of an ethnic nature, minimizing the possibility that these same variations could be due to differing environment, or to differing modes of adaptation of the culture to its environment.

In this respect, the investigation of one variant of Neolithic and Chalcolithic (Eneolithic) technology which obviously does not have any ethnic unity might be of interest. This is the so-called microlithic flint industry of the Asiatic USSR and of China and Mongolia (Fig. 1). Material of this sort first appeared in quantity during the second decade of this century. In those years Soviet geologists, geographers, and other specialists initiated investigations in the desert and semi-desert areas of Kazakhstan and Turkmenia. Collections of flint tools of Mesolithic appearance associated with Bronze Age pottery were made near Ashkhabad, on the Ustiurt plateau, on the Uzboi, and in other regions. These sites came to the attention of archaeologists, and, among others, M. P. Griaznov carried out investigations on the Sagiz river, S. I. Rudenko on Lake Kairan-kul', A. A. Marushchenko in the Aral Sea region, and E. N. Basova on the Man'gyshlak Peninsula.

At the same time very similar sites began appearing in the Far East. On the dunes of Trans-Baikal, microlithic material was gathered by local Irkutsk archaeologists; these collections were studied by G. F. Debets, A. K. Kuznetsov, and S. N. Laptev (Debets 1930; Laptev 1926). In Manchuria, amateur archaeologists also found campsites with microlithic inventories (Lukashkin 1931, 1934; Makarov 1950). A great amount of data on the microlithic cultures of Central Asia resulted from expeditions carried out by Roy Chapman Andrews and F. Bergman in Mongolia, and by K. Hamada, S. Mizuno, and N. Egami in northern China (Teilhard de Chardin and Pei 1944; Maringer 1950). As in Kazakhstan, in these areas also flints of Mesolithic appearance were associated with bifacially worked arrow points and with pottery.

If it were not for the fact that these investigations have either been unpublished (Andrews expedition) or scattered in the literature, we might already have dealt with the problem of the microlithic culture zone in the Asiatic steppe and desert. Recently this situation has changed for the better, since new publications have appeared on the microlithic campsites of Kazakhstan based on large new collections (For-

mozov 1949, 1950a, 1950b, 1951). A. P. Okladnikov has investigated a series of sites in Mongolia; Pei Wen-chung and Yin Ta have issued detailed maps of Chinese microlithic sites (Teilhard de Chardin and Pei 1944; Yin 1956). In Khorezm and Turkmenia, S. P. Tolstov (1948: 59–66) and A. P. Okladnikov (1956a: 205–16) have made not only surface collections, but also excavations in microlithic sites. As a result, we can now more exactly define the Asiatic microlithic zone than we could have ten or fifteen years ago.

What distinguishes the sites of the steppe and desert zones of Asia is not the small size of the implements, since microliths may be found in many other regions. The most important trait appears to be a highly evolved tool-making technique, in which almost all implements are made on blades and not flakes, with bifacial retouch seldom utilized. All sites with inventories of microlithic type contain splendidly worked prismatic or conical cores with regular facets; many of these cores approach pencil shape. Great quantities of blades of all sizes are found, from large examples 2 cm. in width to tiny ones 1 mm. wide. The most common blades are 7 to 10 cm. long and 5 mm. wide. The blades often have retouched edges, converting them into knives. Other tools were also made on blades. First of all, there are end scrapers made on a section of blade 1.5 to 2 cm. long; longer ones are made of complete blades. Burins are blades with a sharp cutting edge formed by a blow directed on the blade at an angle to its side. Spokeshaves are blades with a concave working edge; microblades were incorporated into composite tools. Small blades occur with retouch on one edge or on the end of the blade; this retouch is as a rule for the purpose of blunting. Comparatively rare types of blade tools include pointed awls, gravers, and drills (Figs. 2, 3).

Occasionally two kinds of arrowpoints were manufactured from blades. The first is a single-shouldered point, well known in sites of the Kel'teminar culture in Khorezm and Kazakhstan; points of this nature are also found in Siberia (Figs. 2 *a, b, j, k, p,* 3 *a*). G. F. Debets considers such points as characteristic of the Daurian culture of Trans-Baikal (Debets 1930, Pl. 7, *7*). The second type is sub-triangular with a slightly indented base (Figs. 2 *i,* 3 *g*). While Kel'teminar points are bifacially retouched, the latter type has retouching only on one side. Points of this second type are found in western Kazakhstan (Formozov 1949, Fig. 16), in the Trans-Baikal sites (Okladnikov 1950, Fig. 17) and in China. Tools made on flakes are very rare in the microlithic zone of Asia. Round scrapers made on flakes are found, but they are less frequent than end scrapers made on blades.

The above-described inventory could well be regarded as "Mesolithic," and was so considered by the first investigators in Kazakhstan. However, as we have said, these microlithic flints have everywhere been found with pottery and bifacially retouched arrowpoints of Neolithic and Chalcolithic types. This is not the result of mixed material from eroded sites, as was for a while supposed. The excavations of S. P. Tolstov at Dzhanbas-Kala, and of A. P. Okladnikov in Dzhebel Cave, Kizil-Lai and Kuba-Sengir (Okladnikov 1951a: 80, 89) have demonstrated that within the cultural deposits microliths are associated with pottery. The same situation has been observed in the testing of sites in the northern Aral Sea region, in Trans-Baikal (Debets 1930), in China, and in Mongolia.

It should be mentioned that in desert sites the possibility of the stratigraphic mixing of materials from different periods is much less likely than in the dune sites of the European portion of the USSR. Settlements were always scarce in the desert. Such sites are frequently far removed from water courses, in locations where a shallow well would hit the water table. We investigated this situation in Kazakhstan (Formozov 1950a), both western (Aral Sea region) and eastern (Bet-Pak-Dala and Dzhezgazgan). In these cases it is hard to imagine repeated settlement of a point in the desert which was in no way any more convenient than a thousand alternative ones. Obviously mixed material is encountered only in sites situated in environmentally favorable conditions, that is, on rivers and lakes such as the Ak Suat, where there is pre-Andronovo and Andronovo phase material, and the Ak-Kum-Sagiz, with remains of the pre-Andronovo, Andronovo, Sarmatian and Zolotoordynskii (Golden Horde) phases. Campsites which are far from water courses as a rule give homogeneous material—microlithic flints with Neolithic or Chalcolithic pottery; such an association also appears in stratified sites. Most of the sites of the northern Aral Sea region (Saksaul, Agispe, Sam, those on the edge of the Kara Kum Desert, and others), and of other regions of Kazakhstan, are of this nature.

FIG. 1. Map of northern and central Asia showing localities mentioned in the text. [This map was compiled by Chard — EDITOR.]

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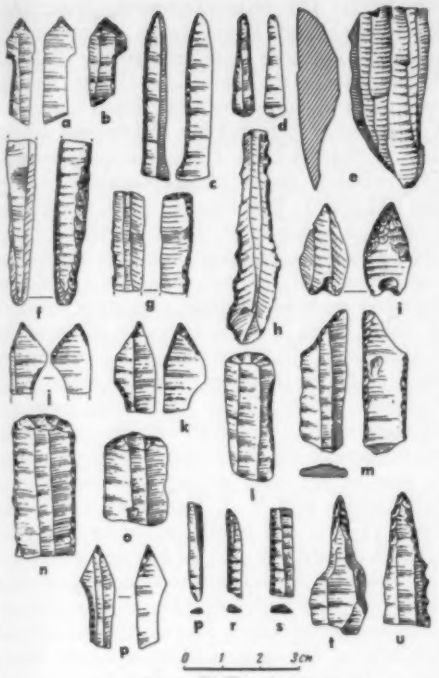

FIG. 2. Microlithic implements from Neolithic and Chalcolithic sites of Central Asia. *a-d*, Dzhanbas-Kala 4, on the lower reaches of the Amu-Dar'ia; *e-g*, Agispe, northern Aral region; *h*, *i*, Kara-oi, western Aktiubinsk region; *j-l*, Besh-Bulak, Kizil-Kum Desert; *m-u*, Saksaul, northern Aral region.

The above observations as well as data from I. V. Sinitsyn's excavations on the Volga (Berezhnov II cemetery), from those on the left bank of the Dniepr (Bader 1950), and in Crimea (Krainov 1957), indicate a persistence of microlithic technology as late as the age of metal, allowing us to place microlithic sites in Asia as being Chalcolithic. Pottery found with microliths in Dzhanbas-Kala, Dzhebel Cave, and a series of eroded sites enables us to date microlithic sites in Kazakhstan and Central Asia in the main to the third and early second millennia B.C. This same chronological placement is indicated by the type of bifacially retouched projectile points—the triangular point with an indentation in the base. Points of this type are common in the third and second millennia B.C. and possibly were manufactured as early as the end of the fourth millennium.

This long persistence of the microlithic technique is a characteristic feature of the material culture of the steppe and semi-desert zones, and is unknown to the north and to the south of this area. Sites of the third and second millennia B.C. in the forest zone of the European USSR and Siberia are characterized by totally different methods of flint working. Here the microlithic technique of making tools from blades did not, as a rule, survive the Mesolithic and early Neolithic epochs. In the later forest Neolithic of the northern European part of the USSR pencil-shaped cores are absent and blades rare; tools here were made on flakes and include knives, scrapers, spokeshaves, and arrowpoints.

In several regions composite implements were manufactured from small blades, as in the Urals and the Lake Baikal area, but blades of medium and large dimensions are almost unknown. Tools manufactured from blades, such as end-scrapers and knives, are also comparatively rare. Oblique points and blades with blunted or notched edge are absent. Bifacial retouch, used in the steppe and desert zone only for the manufacture of a few types of spearpoints, is rather widespread here. The Ural and Lake Baikal assemblages include arrowpoints and spearpoints, knives, small axes, and chisels, all with bifacial retouch. In a series of sites in the Urals (Elovyi Mys, Nizhniaia Makushka near Sverdlovsk, Bor II on the Kama, and others) even such tools as scrapers were subjected to bifacial retouch (collections in Sverdlovsk and Perm universities). In Siberia there are many bifacially worked side blades for

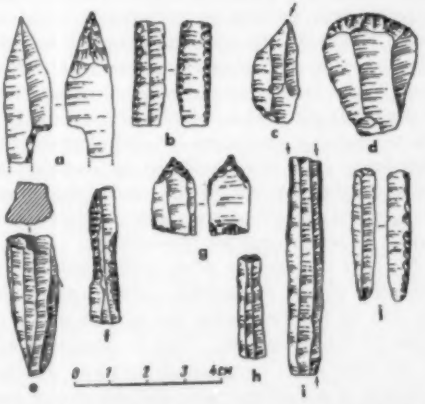

FIG. 3. Microlithic implements from the Trans-Baikal region. *a-e*, Ust'-Kiakhta (collection of G. F. Debets in the Anthropological Museum at Moscow University); *f-j*, Khutor Storozhevskii.

FIG. 1. Map of northern and central Asia showing localities mentioned in the text. [This map was compiled by Chard — EDITOR.]

daggers and knives. Finally, throughout the forest zone bifacially worked or polished heavy tools are found, including axes, chisels, and adzes.

We see a different picture to the south of the area with microlithic assemblages. In sites with painted pottery (Anau, Yang-shao) the stone inventory consists of heavy tools such as axes, chisels, and hoes, but other lithic materials are rare here. Apparently they were squeezed out by metal tools.

All this demonstrates that the stone inventory of the Neolithic and Chalcolithic epochs in the steppe and desert parts of Asia is unique. For this reason those doubts expressed by A. P. Okladnikov as to the reality of a microlithic culture zone seem to us unnecessary. In citing the absence of the microlithic techniques in Siberia, China, and Mongolia (Okladnikov 1955: 105), he apparently has in mind the absence of geometric forms in this area. However, the terms "microlith" and "geometric" are not synonymous. All Paleolithic specialists agree that a process of microlithization took place during the Upper Paleolithic (Boriskovskii 1953: 398, 402–4); however this process does not necessarily entail the appearance of geometric tools, but rather a development of a technique for detaching blades from a prismatic nucleus and manufacturing tools from these blades.

Okladnikov (1951b: 173) further states that heavy tools were more important than light ones to the Neolithic desert dwellers. This is arguable, as the great majority of sites in Kazakhstan and Turkmenia lack heavy tools entirely. Secondly, the important thing is not the relative value of one or the other type of tool in daily life, but rather the regional distribution of the blade technique, which survived in some places and not in others at the close of the Mesolithic. For this reason it is necessary to maintain the concept of the microlithic zone. The general and all-embracing term "Neolithic" cannot be substituted for this concept.

Another question is what is implied by the existence of the microlithic culture zone. As we said in the beginning we cannot see it in terms of ethnic groups. This is demonstrated by the very exact congruence between the distribution of microlithic sites and the steppe and desert zones. To the east of the Volga, the sites of interest to us are widely distributed in Kazakhstan from the regions of the Uzen' (Spitsyn 1950), Uil (Kum-Zhargan), Sagiz (Ak-Kum-Sagiz) and Emba rivers (Kok-Tubek), across the northern Aral Sea territory (Saksaul, Agipse, sites in the Kara-Kum Desert, Aral'sk, Sam; map in Formozov 1951, Fig. 1), and the Turgai region (Chernikov's collections) all the way to Bet-Pak-Dala (Bulat-Talu, Kent-Aral, Chinly-Bulak; unpublished collections of Selevin, IAkovlev). But the areas south of the Ural foothills, a region of scattered islands of forest, have, as well as the microlithic sites of Ak-Suat and Dzhar-Kul', others of quite a different character with assemblages consisting basically of flakes, with a large quantity of bifacially worked tools, among which are heavy scrapers made on flakes, choppers (Tersek-Karagai, Kol'; Formozov 1950b: 70–2). Microlithic traits are also rare in the Semipalatinsk sites situated on the borders of the forest-steppe zone. This can be seen in the early collections made by the Belosliudovs and V. I. Kamenskii (Formozov 1951: 13–14), and in the materials excavated by S. S. Chernikov at Ust'-Narym, which are characterized by numerous bifacially worked tools, flint side-blades of Siberian type, knives, drills, and arrowpoints. The recently-uncovered Neolithic of the upper Ob has a wholly forest aspect (Komarova 1956). The Yenisei Neolithic is poorly known, but artifacts from the Krasnoyarsk region (Okladnikov 1957), and in the collections of S. A. Teploukhov from the Minusinsk basin (Hermitage Museum) are not congruent with the Kazakhstan sites. Here, river pebbles were widely utilized for the manufacture of crude implements; there are many large scrapers made from flakes, but no end-scrapers made on blades. Thus the northern border of the microlithic province does not penetrate into the forests and mountains (Altai, Salair).

To the south of the western Kazakhstan sites, microliths are widely distributed in the Kizil-Kum Desert (Besh-Bulak, Liavliakan), in the Kara-Kum Desert, on the Uzboi (Charyshly, Kugunek, Bala-Ishem), in the lower and middle reaches of the Amu-Darya (Dzhanbas-Kala, Tadzhi-Kazgan; Vinogradov 1957), and finally in Turkmenia (Dzhebel, Kizil-Lai, Kuba-Sengir, Kailiu, Dam-Dam-Cheshme; Okladnikov 1951, 1956a). But microlithic flints are absent in the mountain regions to the south of a line extending from Samarkand to Tashkent, Dzhambul, Frunze, and Alma Ata. The Neolithic and Chalcolithic material recently found in Tad-

zhikistan by A. P. Okladnikov and called by him the Gisar culture has quite a different appearance, consisting mainly of crude tools made of river pebbles.

The second region of distribution of microliths in the USSR is Trans-Baikal. According to G. F. Debets the whole region to the west and north of Lake Baikal lacks microlithic sites. Sites of the microlithic Daurian culture are known only to the southeast of Lake Baikal, in the valleys of the rivers Uda, Selenga, Chikoi, Onon, Ingoda, Ili, and Kerulen (Debets 1930). The recent investigations by Okladnikov in the Angara region and along the Lena river do not alter these conclusions. The zone with which we are concerned is precisely where the mixed deciduous and pine forests of the Selenga, Shilka, and Argun' rivers interfinger with the steppe (Suslov 1947: 299, maps 6 and 8 in appendix). This area is contiguous to Mongolia and Manchuria, and not too distant from the Great Wall, where microlithic sites are also found in the steppes and semi-deserts. Connecting the two microlithic areas of the USSR is the Sinkiang region, where microlithic material is known from the collections of Aurel Stein (1928, Pl. 22). Unfortunately we almost completely lack data on the microlithic industries of the intermediate region of Southeastern Kazakhstan. Thus microlithic sites are not found in forest or mountain zones. This seems to indicate an ecological, not an ethnic, basis for the phenomena which concern us here (Fig. 4).

A similar conclusion can be drawn from other archaeological material associated with microliths. The pottery associated with microlithic flints differs in the various areas. Western Kazakhstan and Khorezm have pottery of the Kel'teminar type with incised and dentate decoration. Pottery of this type is absent in Turkmenia. The cave sites of Dzhebel, Dam-Dam-Cheshme II, Kailiu and the campsite of Kizil-Lai are characterized mainly by plain dark ceramics (Okladnikov 1953: 30–1; 1956b: 76–80, 104–8). In regions to the east of the Aral Sea we do not find incised pottery. The designs predominating here were applied with a dentate stamp and closely resemble Ural and Siberian Neolithic techniques of decoration (Formozov 1951: 11–15). This complementary distribution is not the result of chronological differences between western and eastern Asiatic sites as V. P. Chernetsov (1953: 56–7) has proposed. It is unlikely that in western Kazakhstan only early

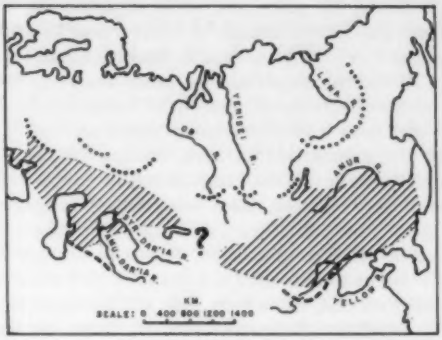

FIG. 4. Schematic map showing the microlithic culture zone during the Neolithic and Chalcolithic. The distribution of microlithic sites during the Neolithic and Chalcolithic is shown by the pattern of diagonal lines. The southern border of northern "Neolithic-holdover" cultures is represented by the heavy dotted lines; the northern border of the painted pottery culture, by the dashed line. This last border in China is given according to Yin (1956).

sites have been found, and in the east only late ones. There are extensive surface collections from the Aral Sea region, Semipalatinsk, and Kustanai which must represent ceramics of all the regional types. As for Trans-Baikal and Mongolia, Okladnikov states that the pottery here is identical to that of the Isakovo and Serovo phases of the Baikal taiga (Okladnikov 1941, Fig. 1; 1951b: 169).

The best witness for the ethnic diversity of this large culture area is the variation of pottery types found within it. Even the flint assemblages are not altogether identical. As we have seen, there are many traits in common; this similarity leads I. V. Sinitsyn (1952: 63) to conclude that there was an ethnic unity in the Neolithic and Chalcolithic ages between the Volga region and Kazakhstan. However, a closer analysis of the flint inventories of Kazakhstan and the Volga region show some fundamental differences. East of the Urals, the trapezoidal geometric tools so common in the Volga region are scarce. In western Kazakhstan, for instance, trapezoids occur in only five sites, four of which (Kazalinsk, Saksaul, Bashe-Kul', Kairan-Kul') yielded only a single trapezoid each, and one site (Agispe) which had seven trapezoids. The easternmost find of trapezoids in Kazakhstan is Samel'-Kum in Bet-Pak-Dala. The vast majority of Kazakhstan sites—and there are around 200 of them according to S. S. Chernikov—have

produced no geometric tools whatsoever; this area was characterized by other types of composite (inset) flints, that is, backed blades.

A careful inspection of more than 20 museum collections of microliths from the lower Volga and adjacent regions demonstrates that in the geometric-tool area, backed blades are non-existent. One can actually draw the boundary between these two types of composite tools. This boundary lies to the east of the geometric sites on the Uzen' River, to include in the backed-blade area the site of Kok-Tubek on the river Emba, which has Kel'teminar type arrowpoints (Formozov 1950a), those on the Sagiz, the Uil, in the Northern Aral region, and as far as the Kustanai province sites of Ak-Suat and Dzhar-Kul' and the Uzboi sites of Bala-Ishem 9, Kugunek 22, and Charyshly 1.

As we have pointed out in another article (Formozov 1953: 168–72), the microlithic sites on the lower Volga are linked with the Caucasus, where geometric tools are found earliest, and from where they spread slowly north. These Caucasian and Near Eastern sites are related to geometric sites in Turkmenia. From Turkmenia and the Lower Volga geometrics were occasionally introduced into western Kazakhstan, but on the whole Kazakhstan, like Siberia and Mongolia, was characterized by assemblages lacking geometrics. In this connection, the boundary east of the Urals between the two types of composite tools is of interest; this is also the boundary between the Yamno (Pit-grave) and Kel'teminar cultures. Subsequently, approximately the same boundary existed between the Andronovo and Srubno (Timber-grave) cultures. But while both the Lower Volga and Turkmenia had microlithic sites with geometric tools the fates of these cultures were different. The excavations of I. V. Sinitsyn in the Berezhnov II cemetery definitely linked the microlithic cultures of the lower Volga with the Yamno cultures of the third millennium B.C. Here in Kurgan (burial mound) 9, with Burials 3, 5, 9, and 17 were found two microblades, three end-scrapers made on blades, three rounded microscrapers, a composite tool, and two other artifacts quite usual in lower Volga sites. Thus, in the third millennium B.C. geometric tools were still being produced in the lower reaches of the Volga. Another situation exists in Turkmenia. The excavations of A. P. Okladnikov in Dzhebel Cave produced occupations with earlier geometric tools than those found on the Volga, the strata with which they are associated being Mesolithic and early Neolithic. However, in the upper strata of Dzhebel Cave, assigned to the late Neolithic and Chalcolithic, geometrics are missing from the microlithic inventory (Okladnikov 1956b: 194). These tools had already disappeared in Turkmenia in the period contemporary with the Yamno culture. We might also note that lacking in Turkmenia are so-called trapezes with blunted backs, such as are known on the lower Volga in such sites as Raskazinsk, Seroglazovo, Ryn'-Peski, and in the regions lying immediately to the west. Thus, in the microlithic zone, there is similarity neither in the pottery nor in the totality of flint inventory, a weighty argument against ethnic unity.

If this is so what does the microlithic culture of the Neolithic and Chalcolithic periods represent? We must remember that during the Mesolithic, the microlithic zone was much more extensive, being found not only in the steppes and deserts but also in the forest zone of the Volga-Oka-Kama region, and in the mountains of the Caucasus and Crimea.

Investigations of Mesolithic campsites show that their inhabitants led a wandering sort of life, occupying themselves with hunting with the bow and arrow and with collecting. Fishing did not play an important role in the Mesolithic, many sites being located far from lakes and rivers, and fish bones being rare in Mesolithic deposits. Only the very first steps were being taken towards stock-raising and agriculture, and even those not in all regions. Mesolithic man did not undertake large game drives as did Paleolithic man, who by means of mammoth hunts assured himself of a reasonably sedentary life. Mesolithic man hunted non-gregarious animals, probably in peripatetic groups no larger than two or three persons like the modern Australians and Tasmanians. Their hunting success was guaranteed not by the large number of participants, as in the case of game drives, but by the quality of their armament. Superior hunting weapons of this sort included the bow and arrow, throwing weapons made of wood or bone with flint inserts of various sorts, such as backed blades and geometrics. The development of the microlithic technique is correlated with the refinement of missile weapons.

The Neolithic marks a significant change in the economy of early man. In the Caucasus, and in the early cultures of the Near East,

Yang-shao, and others, agricultural societies begin to take shape in the Neolithic and Chalcolithic periods. Heavy tools are now necessary to till the soil and tools like pestles to process the produce. As a result, an assemblage of large coarse tools is added to the microlithic inventory. Such a combination of tools can be seen in Neolithic sites such as Kistrika and Odishi (Lukin 1950), and the site of Jarmo in Iraq (Braidwood 1952). Thus microliths continue through the first stages of the development of agriculture, as shown by finds in Dzheitun (Masson 1957, Fig. 2), Jericho 17–19, Jarmo, and others. However in subsequent times microliths disappear rapidly. This disappearance is correlated with the introduction of copper tools, and with the fact that the spread of agriculture and animal husbandry led to the extinction of hunting and the stone-working techniques connected with it. In developed Anau, and in the Chalcolithic cultures of the Near East, only stone hoes, pestles and the like remain.

Cultural development followed another line in the forest area. Here the Neolithic epoch was the time of the economic dominance of fishing. Numerous finds of harpoons, fishhooks, remains of nets, and fish bones demonstrate that in the forests in the third and second millennia B.C., fishing was more important than hunting. The mastery of fishing techniques allowed these cultures to once again resume a sedentary aspect. In place of the temporary campsites of the Mesolithic, there appear sites with signs of intensive occupation, and remains of dwellings. The necessity to work wood to manufacture rafts, boats, fish-traps and permanent dwellings contributed to the development in the forest zone of heavy tools, such as axes, adzes, and chisels. Thus here as in the southern regions the microlithic technique has lost its importance, although in certain areas, such as Belevsk on the Oka river (Briusov 1952: 53–4) and Lake Andreev in the Urals (Chernetsov 1953: 27) microliths persist for a long time.

In the steppe and desert zone, the onset of the Neolithic did not result in a change in the way of life. There are no indications of agriculture here before the second millennium B.C. Here and there fishing was occasionally practiced, as witnessed by the large quantity of fish bones in Dzhanbas-Kala 4 and by the finds of sinkers in Ak-Kum-Sagiz. However, many sites are found where wells are located or where a shallow pit would reach ground-water (in the northern Aral region, Bet-Pak-Dala, and the Ashkhabad region), demonstrating that fishing could not have been a mainstay of the microlithic cultures in the steppes and deserts of Asia (Vinogradov 1957: 31). Like their Mesolithic predecessors, the Neolithic inhabitants of this area occupied themselves with nomadic hunting. The paucity of cultural material on the sites demonstrates their impermanence. Arrowpoints and spearpoints are frequent; where the stratigraphy is undisturbed there are found wild animal remains including the wild ass (*Equus hemionus*), antelope (*Gazella subgutturosa*) at Dzhebel Cave; boar, deer, roe-deer (*Capreolus* sp.) at Dzhanbas-Kala 4; dzhegitai (a form of wild ass) at Saksaul; and so forth. The first evidence for stock-breeding belongs to the beginning of the second millennium B.C. Sites earlier than this must be regarded as the temporary camps of hunters.

The absence of forest in this region is the reason for the lack of heavy wood-working implements. Finds of this sort in the steppe and desert are exceedingly rare. (Ak-Kum-Sagiz, Rasskasinsk on the Volga). But during the Neolithic there can be noticed a certain retreat from the microlithic technique even in the steppe zone. There appear at this time large, bifacially worked spearpoints (sites in the region of Aral'sk) and arrowpoints. The survival of the Mesolithic way of life in the steppe and desert meant the preservation during the third millennium B.C. of most of the specialties of the microlithic technique. This phenomenon characterizes not only the steppe and desert of Asia but also the left bank of the Dniepr and the Volga region, the area of the Yamno culture. To the west of the Dniepr, among the Tripolye-culture farmers, microliths are unknown. It is of interest to note that a late survival of microliths may be seen in North Africa, where in the desert regions, the Capsian stone industry survives into the Neolithic period due to the lack of change in the economy; the only innovations are bifacially worked arrowpoints (Vaufrey 1936).

Thus, the common features of the flint inventory of such cultures as the Yamno, Kel'teminar, Daurian, and the as yet nameless Neolithic and Chalcolithic cultures of Turkmenia, Kazakhstan, Mongolia, and China do not affirm any such thing as an ethnic unity of these areas. Rather, they mirror the same sub-

sistence pattern in the same ecological area, and the survival of flint-working techniques from the preceding Mesolithic. We therefore are not justified in assuming ethnic ties between the Kel'teminar culture and other regions, where all we have is Mesolithic material or a persistence of the microlithic technique. A comparison of the flint inventory of the Kel'teminar culture with that of the Levshino site in the Urals and of Karim Shahir (Vinogradov 1957: 35) can hardly provide a basis for historical conclusions. It is well known that the Levshino site had a Mesolithic level (Bader 1953), and that a Mesolithic inventory survived into the early Neolithic of Iraq, but it does not follow from this that the Kel'teminar people spread from the Near East into the Aral region and subsequently influenced the Ural cultures like that found in the Levshino site. The lack of geometric tools, typical for Karim Shahir, in the Kel'teminar culture (they do not exist in the earliest site, Dzhanabas-Kala), militates against a genetic tie between these two. It is clear that in the Aral region, as in Turkmenia, there was a local Mesolithic with a microlithic industry. Material of Mesolithic age is now being sorted from old collections by comparison with artifacts from the Dzhebel Cave, where there are stratigraphic data (Okladnikov 1956b: 204–12). The local development of the microlithic inventory of the Kel'teminar culture is much more likely than are speculative migrations based on the similarity, but not identity, of tools from sites which are actually very dissimilar in their pottery.

All these comments do not beg the question of real cultural ties, and possible movements of population both within the microlithic culture zone and beyond it. Chinese archaeologists have discovered a series of sites where one can discern the contact between the microlithic and painted pottery culture zones (Teilhard de Chardin and Pei 1944: 100), ties between which are also indicated by the interesting similarities in design motifs shown by A. V. Vinogradov (1957: 37–42) to exist between Kel'teminar and Anau. We may note in this connection that to substantiate ties between the Kel'teminar cultures and the south one cannot invoke the findings of a *Dentalium* shell in Dzhanbas-Kala 4. The species this shell belongs to is now found in the Indian Ocean, but one must keep in mind that fossil tertiary shells are found on the Sultan-Uiz-Dag, and the Kel'teminar people may have used these fossils, as happened in sites of the northern Aral region (Chokusu). The paleontologist A. G. Eberzin, who has seen the shells from Dzhanbas-Kala, thinks that they are really tertiary *Dentalium*. It is important that the *Dentalium* from Dzhanbas-Kala are larger than the unquestionably local *Didacna*. One may recall also that fossil *Dentalium* used as beads are found in burials from Krivoluch'ia on the Volga, in the Upper Paleolithic burial in Brno, and in many other points. All this makes one think that the find of *Dentalium* in Dzhanbas-Kala is not necessarily an indication of Indian contacts.

It is also possible to trace the influence of the microlithic zone on the northern forests. This is witnessed by the appearance of single-shouldered arrowpoints in the Neolithic burials of the Angara valley. A. P. Okladnikov (1950: 158–62) correctly points out the steppe origin of this type of point, and finds an analogy for it in the Daurian culture. Unfortunately he dates these burials far too early. Assignment to the Mesolithic, or even early Neolithic, is very unlikely, as is indicated by other finds in the same burials (bifacially worked arrowpoints and polished objects) and by the observations of G. F. Debets (1930) on the stratigraphy of sites in Trans-Baikal. The finds on Mt. Chudatsk, at Kuium in the Altai range (Kiselev 1949: 13–14), and in the sites described by us in the Kustanai and Semipalatinsk regions, likewise probably reflect the influence of the southern microlithic inventory on the cultures of the forest-steppe where there was a different way of life.

It is certain that there are, even within the boundaries of the microlithic zone, intensive cultural ties, since there are no barriers to such in a steppe environment where widespread hunters would have had extensive contacts. In this connection, the problem of the origin of the eastern microlithic sites is especially interesting. It should be stressed that the Siberian Mesolithic, not distinguished by investigators from the local Upper Paleolithic, does not have the microlithic character of the Mesolithic sites in Europe and Central Asia. An industry based on the working of river pebbles, the production from these of large rounded scrapers, scrapers on flakes and other artifacts, could not, it seems, have been the predecessor of the microlithic cultures of Trans-Baikal and Mongolia. On the contrary, the development of this late, Paleolithic-looking industry may be seen in the Taiga

Neolithic of Siberia. Associated with the Isakovo burials of the Angara valley are scrapers of Paleolithic appearance (Okladnikov 1950, Fig. 30); finds in Biriusinsk Cave and in several other sites show that the Paleolithic character of stone-working persisted in Siberia until the advent of pottery.

On the other hand, one may recall the astonishing similarity of two types of arrowpoints made on blade from the Daurian and Kel'teminar cultures; the finding in Trans-Baikal of tools resembling the western trapezoids with blunted backs (Debets 1930, Pl. 7, *7*); and the North Chinese pottery published by Japanese archaeologists, which was ornamented with a "walking comb" (dentate rocker?) and is very close to Kazakhstan ceramics (Hamada and Mizuno 1938). These similarities lead us to pose the question, are there reflections of the Kazakhstan Neolithic in Trans-Baikal and Manchuria? No final answer can be given before large scale excavations are undertaken in Kazakhstan and Siberia, but it is a good question all the same. It may be that this question is connected with that of the Europeoid admixture in the Neolithic population of the Angara, to which G. F. Debets (1948: 61) called attention.

While future excavations may prove or disprove particular points in this discussion they will not alter the general position about the environmental unity of the microlithic culture of this zone as a whole. Before us we have, in the language of ethnography, not a "historico-ethnographic" but a "culture-adaptation" area (Levin and Cheboksarov 1955).

BADER, O. N.

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RECENT DISCOVERIES IN THE ATACAMA DESERT AND THE BOLIVIAN ALTIPLANO

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ABSTRACT

Recently discovered paleo-Indian sites on the shores of Salar de San Martín in Chile and Laguna Colorada and Laguna Hedionda in Bolivia are described. A cave at Laguna Hedionda is stratified. Comparisons are made with Ayampitín in Argentina and Le Paige's sequence for sites around Salar de Atacama in Chile. Pottery from the Hedionda sites is compared with post-Tiahuanaco to Inca styles in nearby areas of Bolivia. Le Paige's sequence (Puripica, Tulán-Cebollar, Tambillo) is criticized and a revision (Puripica-Tulán, Tambillo-Cebollar) proposed.

IN JULY and August 1958 a group of four students from the University of Cambridge (England) carried out an archaeological and geomorphological survey in the eastern part of the Chilean Province of Antofagasta and the adjacent altiplano in the southwestern portion of the Bolivian Province of Potosí (Fig. 1). The expedition was principally concerned with the study of the Chilean salt lakes (*salares*) and the small lakes (*lagunas*) in the Bolivian altiplano (Vita-Finzi 1959).

The *salares* of the Atacama desert form a rough chain along the western margin of the Andes, while the *lagunas* are concentrated in a similar chain, high up in the mountains farther to the east. The River Loa, the only river to cut through the desert from the Andean watershed to the Pacific Ocean, rises in this part of the Andes and with its tributaries, the Salado and the San Pedro, is the other great drainage feature of the region.

THE ATACAMA DESERT

The upper reaches of the Loa and the oases on the east shore of the vast Salar de Atacama have long been known to have been the center of a flourishing pre-Columbian Indian community whose descendants, now mixed with European blood, still live there. The material remains of these Atacameño Indians have been remarkably preserved by the arid climate and numerous studies have been carried out which are adequately summarized by Bennett (1946).

In the last few years a completely new and much earlier field of the prehistory of the Atacama desert has been opened up by the discovery of numerous sites belonging to a paleo-Indian stage of culture. So far these have nearly all been found in the region of the *salares*, between Ollague in the north and the Salar de Atacama in the south. They represent the temporary encampments of nomadic hunting groups. Blanks for large ovate and oblong core implements have been found at a prehistoric stone quarry and chipping floor on the shores of the Salar de Talabre, near Chuquimata (Barfield 1960).

Gustavo Le Paige, S. J., of San Pedro de Atacama has identified several distinct cultural groups by studying a collection of stone artifacts found on a large number of sites (Le Paige 1958). He has recognized four main sites to which most of the finds from other parts of this large area can be related, and has also noted other more nebulous traditions. These groups are named after the type sites and each has a more or less recognizable geographic distribution. Le Paige has attempted to fit them into a chronological framework based partly on typology and partly on a comparison with the more accurately dated cultural sequence in northwest Argentina. His four main groups are Puripica, Tulán, Cebollar, and Tambillo.

Puripica. Sites of this tradition are found over most of the area, but are notably concentrated on the streams which flow in at the northeast corner of the Salar de Atacama. Characteristic artifacts are the large leaf-shaped (Fig. 5 *a, d-f*) points which have been found in great numbers, mainly broken, at the type site. At Guatín, an extensive quarry area, other forms were found, including knives (Fig. 7 *d*), awls, and also some points which belong to the later Tambillo stage. The material from which these artifacts is made is usually coarse basaltic rock which accounts in part for their rough character.

Grinding stones are found on some sites and at least when they occur on one-period sites such as Calar they can be attributed to this phase. Dry stone walled shelters called *pircas* have been found occasionally and at Calar 25 circular houses and two large corrals, presumably for animal roundups, are associated with a rich Puripican industry. Le Paige regards the Puripican as the earliest group on account of the roughness and size of the points, probably

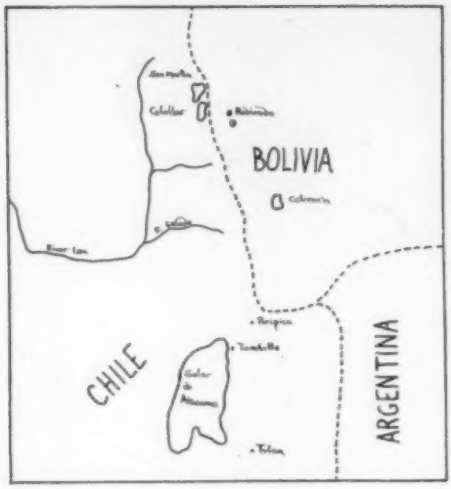

Fig. 1. Sketch map of northern Chile and southern Bolivia.

spear heads, and the evidence of the presence of sites of the typologically more advanced Tambillo group found in almost exclusive distribution in the same region.

Tulán. This second group has only been found at the south end of the Salar de Atacama. Large flat finely flaked points are common, made of a fine-grained beige volcanic rock (Fig. 2). Le Paige regards this group as later than Puripica on typological grounds.

Cebollar (also referred to as the Northern Group). The only characteristic artifact of this group is the small triangular and sometimes hollow-based arrowhead (Fig. 11 *h*). Almost identical assemblages of great quantities of these basaltic stone points are recorded from the shores of the northern *salares*, Ascotán, San Martín, and Uyuni (Bolivia). According to Le Paige this group is contemporary with Tulán.

Tambillo. Sites of this group are concentrated mainly on the eastern shores of the Salar de Atacama. The stone industry includes a wide range of projectile-point types mostly tanged but also triangular, hollow-based and oval. Most diagnostic of this culture however is the winged lozenge-shaped or "tetragonal" point. Other characteristic artifacts are oval scrapers and steeply backed awls. The makers of these artifacts appear to have gone to some trouble in collecting a wide range of colored volcanic rocks to use as raw material. Large numbers of stone pestles and hollow-ground mortars were found on the type site.

Le Paige fits these four groups into a three-phase chronological system which is not entirely convincing. The sequence begins with Puripica, is followed by the contemporaneous Tulán and Cebollar groups, and ends with Tambillo.

On the shores of the Salar de Atacama the more elaborate Tambillo assemblages are clearly later in date and distinct from those of Tulán and Puripica. At Pelun, for example, pure Tambillo and Puripica sites were found side by side while on the southern shores of the *salar*, Le Paige has recorded sites which he regards as transitional between Tulán and Tambillo. The only evidence other than typological, however, for the priority of the Puripica stage is provided by two pairs of sites, one at Yona the other Tulor and Tebinquiche, where Puripica assemblages were found higher up the shore of the *salar* than those of Tambillo. It is possible that here we have evidence of successive settlements on the shore of a shrinking lake.

The Tulán and Puripica groups on the other hand appear to have a complementary distribution and could in fact be to some extent contemporary. Similarly the Cebollar sites in the north have a complementary distribution to the

Fig. 2. Typical point from Tulán.

southern Tambillo group. This together with the fact that hollow-based arrowheads of northern type are found at Tambillo suggests the equation of Cebollar with Tambillo rather than Tulán. Thus at the moment instead of Le Paige's three-stage sequence it is perhaps prudent only to postulate two chronological stages: an early Puripica and Tulán stage, followed by Tambillo and Cebollar.*

For absolute dates Le Paige resorted to the similarities between Tambillo and the Ayampitín culture of Argentina dated by radiocarbon at 6013±100 B.C. (Barendsen, Deevey, and Gralenski 1957, Y–228. The equation of Tambillo with Ayampitín is not certain and in fact on present evidence it could just as well be argued that Puripica was coeval with Ayampitín.

The two most interesting paleo-Indian sites that the Cambridge expedition found in Chile lay on low volcanic hills on the northeast shore of the Salar de San Martín. The largest of the two sites (San Martín II) occupied a large part of the flat summit of one of these hills. Here an abundant stone industry was associated with a group of three rough circular shelters constructed of tufa blocks (Figs. 3, 4). The largest hut was composed of three inter-connecting rooms with an entrance at the north end. The internal measurement of the longest axis of this hut was approximately 22 feet and the height of the walls varied from about 1 to 4 feet. The other two hut shelters were horseshoe shaped with entrances to the east and internal diameters of 6 and 4 feet.

Most of the stone artifacts were found scattered round the largest hut and most characteristic of these were the leaf-shaped points which were usually broken and only rarely bifacially worked (Fig. 5 *a*, *d-f*). Scrapers and cores were also found. The black basalt of which these implements were made is not found naturally on the site and was probably imported from the basalt boulder screes on the ridge between Ascotán and San Martín where there is considerable evidence of prehistoric quarrying. This assemblage with its rough leaf-shaped points is closely paralleled at Puripica sites and the circular huts of *pirca* construction recall those found at Calar by Le Paige (1958: 160).

* After this paper was sent to press, I received Le Paige's second publication (1960) in which he presents the two-phase chronology suggested here.

FIG. 3. Hut B at San Martín II, taken from the east. Scale of inches is on the right side of the entrance.

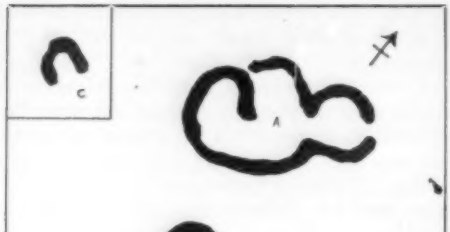

Fig. 4. Sketch plan of *pircas* associated with Puripica industry at San Martín II. Inset C is approximately 50 yards northwest of the main group.

The other site (San Martín I) lies about 1 km. to the west of San Martín II. A small scatter of basalt Puripican artifacts were here found on the side of a volcanic hill at a height of about 40 feet above the floor of the *salar*.

The fact that these two sites were both 40 feet above the *salar* floor and that sites of the later Cebollar group are almost always concentrated along the margin of the salt flats agrees with Le Paige's observations at Yona and at Tulor and Tebinquiche and argues for the existence of the Puripica group while the *salares* were still lakes. This fact is not confirmed, however, by the Bolivian evidence where Puripican sites have been found on the present day margin of the *lagunas*. Moreover, it would mean the sites were islands when occupied.

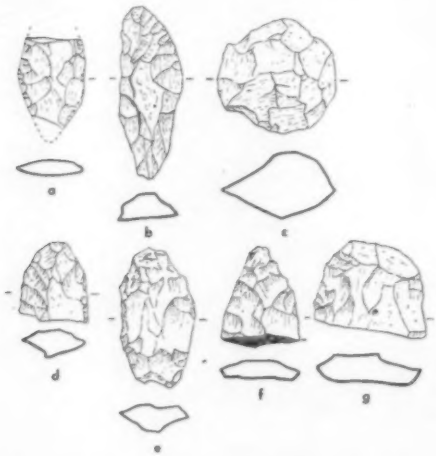

Fig. 5. Black basalt implements of Puripica type associated with hut circles at San Martín II.

The Bolivian Altiplano

Laguna Colorada. This lake occupies the northern part of a flat volcano-encircled basin lying about 40 km. to the east of the source of the Río Salado. It is fed by one major stream entering from the east and by smaller streams and volcanic springs along its northern shore. Pink microfauna in the lake supports large flocks of flamingos (Conway 1961) and other birds; vicuña, vizcacha, and large rats are frequently seen on the shores. There is no permanent Indian settlement on the lake today although a seasonal encampment on the sheltered northern shore at Solor is visited by local Indians in December when they collect the flamingo eggs. The site is also used as a resting place by travelers crossing between Bolivia and Chile.

Small numbers of artifacts were found at eight sites round the lake, mostly along the well-sheltered and watered north shore (Fig. 6). Together they give an indication of a long tradition of hunting in the area, but at present can only be tentatively fitted into a cultural or chronological scheme. According to Le Paige's chronology the earliest material is that from Solor, where there are implements of his Puripica stage (Fig. 7 *a*, *b*) including a serrated point resembling an Ayampitín type (Fig. 7 *c*). These were surface finds as on all the other sites and here were mixed with egg shells and other modern rubbish. Flakes and a knife similar to ones from Guatín were found at Site VI south of the laguna (Fig. 7 *d*).

Fig. 6. Sketch map of Laguna Colorada.

The majority of the sites along the north shore of the lake appear to belong to a related cultural tradition quite distinct from any of the main Chilean groups. Le Paige has in fact compared some of the Colorada material with his rather nebulous Central group of paleo-Indian culture which is centered on the Río Salado not far to the west of Laguna Colorada (Le Paige 1958: 164). The points of this Colorada group are relatively small and leaf-shaped. Two recurrent forms are a slender point (Fig. 7 *f*, *g*) and a stumpy "awl"-tipped point (Fig. 7 *e*, *k*). The only other characteristic implement is a gouge made from a bifacially worked blade by having a longitudinal flake removed from one end, a technique identical to that of the Levallois tortoise cores (Fig. 7 *j*, *m*). At Site II the stone tools were mixed with presumed Atacameño sherds, and at another site, with glazed ware of postconquest date. At Site VII, which lay among the strangely eroded cliffs on the south edge of the basin, was a working floor, which yielded an oval scraper and a leaf-shaped point both with stepped flaking (Fig. 7 *n*, *o*). These differ in type and raw material from any other tools found in the region and are perhaps later in date than the above mentioned paleo-Indian material. Some dry stone shelters and recent potsherds were also found nearby.

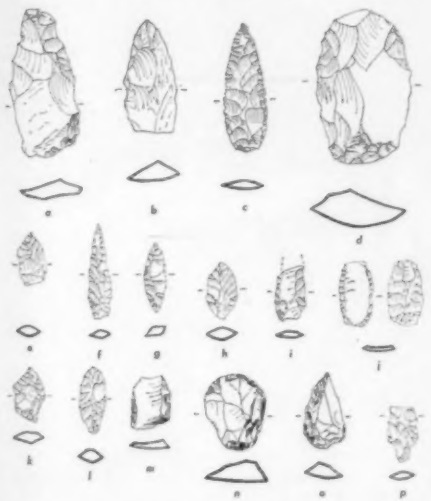

FIG. 7. Stone artifacts from Laguna Colorada. *a-d*, Puripica group: *a-c*, Solor; *d*, Site VI; *e-m*, Colorada group: *e*, Site I; *f*, *g*, Site III; *h-j*, Site IV; *k-m*, Site V; *n*, *o*, other traditions, Site VII; *p*, an isolated find. Length of *d*, 8.3 cm.

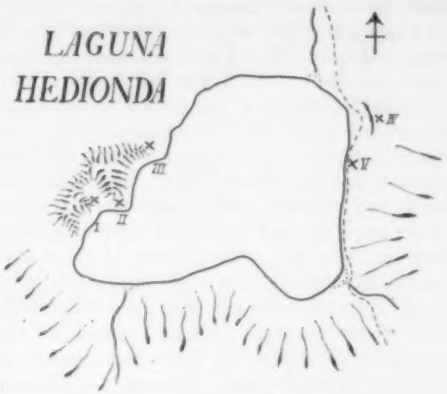

FIG. 8. Sketch map of Laguna Hedionda.

Finally, mention should be made of a small tanged obsidian arrowhead from Solor which closely resembles those from Atacameño Indian sites to the west and also the isolated broken tanged point (Fig. 7 *p*) found on the banks of the main tributary stream which is similar to points associated with painted pottery discovered at Peña Barrosa further east in the Bolivian highlands.

Laguna Hedionda. The second lake that was studied lies about 70 km. to the north of Laguna Colorada. It is very much smaller, being only about 500 m. in diameter, but around its shores were again found several camping sites which showed signs of occupation from very ancient to recent times (Fig. 8). Of the four main sites three (I, III, IV) had been occupied at a number of different times down to the present day. Site V, however, was a one-period encampment and clearly the earliest. Here numerous flakes and broken Puripica points of black basalt were scattered near the water's edge (Fig. 11 *a-d*). The only other tool type was a gouge similar to the Colorada examples (Fig. 11 *e*). Points resembling those from Tambillo were collected from Site III (Fig. 11 *f*, *g*) and from below the mouth of the cave, Site IV (Fig. 11 *s-v*).

The stratified deposits inside the cave (Fig. 9) had been almost completely cleared out in recent times, but fortunately a wall of original filling had been left undisturbed as a partition wall between two parts of the cave. Excavation of part of this section revealed 12 layers of stratified cave occupation and volcanic dust deposits (Fig. 10). The lowest deposit, Layer 12, contained minute flakes of basalt, the refuse

from the secondary flaking of stone implements. In Layer 11b these were accompanied by artifacts of basalt (Fig. 12 *a-c*) and chips of obsidian and quartz. The most important artifact from this layer is a triangular hollow-based arrowhead (Fig. 12 *b*). Two backed blades were found in Layer 11 (Fig. 12 *d*). Above this only a few flakes were found in the layers of cave earth and rubbish, while near the top these layers are composed largely of matted flamingo nests and feathers. Above Layer 11 these cave occupation deposits are interleaved with fairly thick layers of clean volcanic dust which indicates fairly intense volcanic activity in the neighborhood during the human occupation of this part of the Andes. Animal bones represented wild species of the llama family, rodents and birds, still to be seen around the lake today. A broken human humerus was also found. The triangular hollow-based arrowhead is typical of the Cebollar group whose nearest pure sites are on the Salar de Ascotán to the west, although similar triangular points were found at the mixed Site III not far from the cave.

At Site III besides the points that can be best paralleled at Tambillo (Fig. 11 *f, g*) and Cebollar (Fig. 11 *h*) was another group of stone points (Fig. 11 *i-n*). These are tanged or oval, very small and almost always made of a light green volcanic stone. These are unparalleled elsewhere and probably later than the other paleo-Indian remains. They may in fact be associated with the incised gray pottery found on the same site. White stone knives found at Sites III and I (Fig. 12 *f*) are almost identical to one found on a "non-pottery" site at Chacilla on the Bolivian Río Grande to the east. These together with small tanged arrowheads from Site I (Fig. 12 *g*) are probably relatively late in date.

Le Paige's scheme thus seems applicable in part to this southwest corner of Bolivia. The

Fig. 9. Cave with stratified volcanic dust and occupational debris at Laguna Hedionda IV. Modern Indian windbreaks protect the entrance.

Puripican stage is represented at both lakes by encampments on the present shore line which contrasts with the high level sites around the *salares* even though high strand lines also are present around the Laguna Hedionda. A mixed Tambillo-Cebollar tradition is found around Hedionda but at Colorada a different group, perhaps related to Le Paige's Central Group on the Río Salado, is found. The later traditions of stone working are probably more closely related to Bolivian cultures of the altiplano to the east than to Chile, where at present no comparable finds have been made.

At three of the Hedionda sites (I, III, IV), besides the stone artifacts, there were a number of sherds of prehistoric and modern pottery. The coarse wares showed no very distinctive forms and it was not possible to date them although it is fairly certain that the heavily mica-tempered sherds from Sites I and III are of relatively recent date. Three groups of finer prehistoric wares are recognizable: Incised Gray ware, Dark Brown Painted Buff ware, and "Llamita" ware.

Incised Gray ware is only represented by two sherds from Site III: a bead rim of burnished gray-and-red mottled ware, decorated with oblique scratched lines after firing (Fig. 10 *h*); and a burnished gray sherd incised and impressed before firing (Fig. 10 *i*). No similar sherds were found on any of the other sites nor are there any immediate parallels in the north of Chile. The second sherd, however, bears a close resemblance to pottery from the Mizque valley several hundred miles to the east in southeast Bolivia. This is the so-called Mizque gray ware which has incised and dotted decoration forming zig-zag motifs and pendant triangles (Rydén 1956). If this comparison is valid we can accept Rydén's dating of the *Muzque* ware to the interval between Tiahuanaco and the Inca periods as applicable to our two sherds.

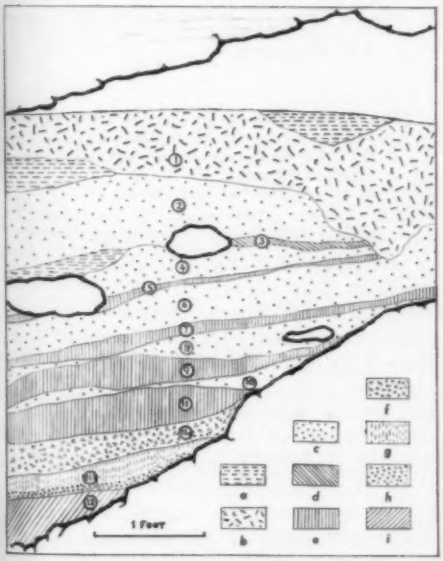

FIG. 10. Cross section of stratified deposits in cave at Laguna Hedionda. *a*, flamingo nests; *b*, stained volcanic dust and nest debris; *c*, clean volcanic dust; *d*, rock fall, nest debris, and charcoal; *e*, sand and animal and human refuse; *f*, sand with rock fragments; *g*, brown-stained volcanic dust; *h*, vizcacha excreta; *i*, dark brown earth.

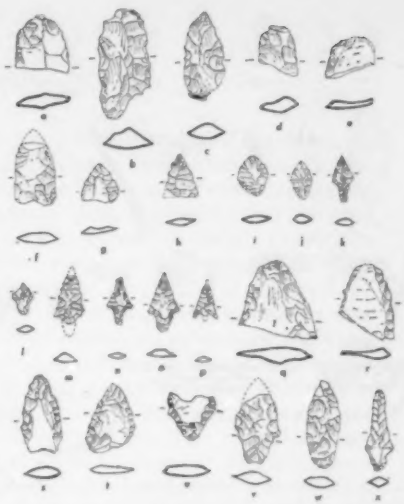

FIG. 11. Stone artifacts from Laguna Hedionda. *a-e*, Puripica type, Site V; *f-p*, Site III: *f*, *g*, Tambillo type; *h*, Cebollar type; *i-n*, Hedionda points; *q-x*, surface collection from Site IV. Length of *b*, 6.5 cm.

Painted bowls of a Dark Brown Painted Buff ware were found at the three Hedionda Sites, I, III, and IV, and also at Peña Barrosa, a small village to the east of Laguna Colorada. The ware is usually a pink to buff color and the surface is covered with a burnished buff slip. The sherds are all of small rounded bowls with a simple upright rim. The decoration is normally restricted to the inside and outside of the lip although occasionally the lower parts are painted. There are several painted motifs.

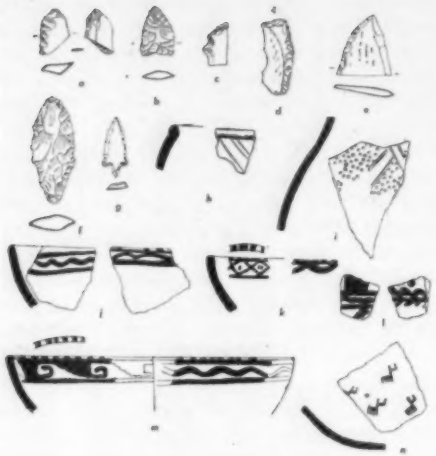

Fig. 12. Artifacts and pottery from Laguna Hedionda. *a-e*, stratified artifacts from Site IV: *a-c*, Layer 11b; *d*, Layer 11; *e*, Layer 9; *f*, *g*, Site I; *h*, *i*, Incised Gray ware, Site III; *j-n*, Painted ware, *j*, *k*, Site I; *l*, Site III; *m*, *n*, Site IV. Length of *f*, 6.2 cm.

A wavy line enclosed by two straight parallel lines is the commonest motif and was found at all sites either in horizontal bands or else as rim festoons (Fig. 12 *j*, *k*, *m*). The same motif appears on Inca period pottery at Palli Marca in north Bolivia and also on vessels from the Chilean Loa valley in Atacameño culture contexts (Rydén 1944, Fig. 79 *w*, from Chiu-Chiu where the bordered festoon is found on both sides of the rim). A chain of lozenges filled with dots is found only on Site I (Fig. 12 *k*). It is paralleled in the north on a bowl from a dwelling site between Palli Marka and Khonkho Liquilqui (Rydén 1947, Fig. 98). The straight-sided running spiral, found only at Site IV (Fig. 12 *m*), is a favorite textile pattern still used in south Bolivia today. Branch designs and thick vertical bands were only found in Site III (Fig. 12 *l*). The top of the rim itself is often painted or striped with bands of brown paint (Fig. 12 *j*, *k*, *m*). The banded rim is also found in the north of Bolivia on "Llamita" bowls. This dark brown painted ware is apparently a Bolivian product closely related to Rydén's painted ware in the north where it is attributed to the post-decadent Tiahuanaco tradition of the Colla Indians before and during the Inca domination.

One sherd of the very characteristic "Llamita" ware was found at Site IV (Fig. 12 *n*). It is a sherd of gray ware with a light red slip inside and a reddish brown slip on the outside. The inside is painted with black, stylized, four-legged llamas. This is a well-known type of pottery called by Rydén "Llamita" ware and is a characteristic product of the Bolivian Colla Indians during Inca times. These pots have a wide distribution throughout Chile and Bolivia (Rydén 1947). These Hedionda painted llamitas differ from those excavated by Rydén at the Inca sites of Palli Marca and Cchancha del Kjula Marca in having four legs, whereas those from the north only show two.

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Universität Bonn
Bonn, Germany
December, 1960

LEOCADIO E. HOPUN

1894–1960

Maya archaeology and Maya archaeologists lost a loyal friend with the death, on November 24, 1960, of Leocadio E. Hopun in El Cayo, British Honduras. In the 1920's and 1930's, when the best means of access to the eastern Petén Department of Guatemala was by way of British Honduras, with equipment and supplies shipped by river from Belize to El Cayo and there transferred to mule trains for the journey into such places as Uaxactún, Tikal, or Naranjo, a key person in every archaeological organization was the expedition's agent in El Cayo. He made all the arrangements and kept a field party's life-lines maintained for the three or four month period in the bush. Leocadio Hopun, by profession an accountant and Cayo businessman, performed in this role of agent with excellence. Efficient, vigilant, he saw to everything. He acted as agent for the Carnegie Institution of Washington during its long program at Uaxactún. Again, after the second war, he served the University of Pennsylvania and Harvard University in the same capacity during their surveys and excavations in British Honduras and Guatemala. This is to name only a few of the scientific groups aided by Hopun over a period of almost 40 years. His friends will remember him as a man of keen intelligence, familiar with the country and with its people, and interested in the cause of archaeology. It will be difficult, indeed, to find someone to take his place.

A. Ledyard Smith
Linton Satterthwaite, Jr.
Gordon R. Willey
William R. Bullard, Jr.

KARL RUPPERT

1895–1960

The sudden death of Karl Ruppert, at Rochester, Minnesota, on August 14, 1960, came as a great shock to many. He had gone to the Mayo Clinic, hoping to gain relief from an arthritic neck condition, which had been causing intense pain for nearly a year. Only two days before death struck, friends in Santa Fe had received cheerful notes from Karl, in which he said he was without pain for the first time in months. Death was caused by peritonitis. Ruppert was born in 1895 in Phoenix; his only survivor is a brother who lives in that city. His first academic work was at the University of Arizona, where, fortunately, under the tutelage of Byron Cummings, he was bitten by the archaeological bug. As Cummings' assistant, he was a partner on many explorations in the Kayenta region.

His first important assignment was in Chaco Canyon, with Neil Judd. Here he was in charge of the excavation of Pueblo del Arroyo, during 1921–26. Some of his happiest memories were of this period. Ruppert, equally gifted as an architect and archaeologist, was an absolute perfectionist in anything he undertook. He once said that the experience he gained in the Chaco was invaluable to him later. Even after

(Continued on next page)

long association with some of Mesoamerica's most impressive stone cities, he still retained his respect for the masonry edifices of the Chaco.

In the winter of 1925, he joined the staff of the Carnegie Institution of Washington, and for the next 32 years, dedicated himself to Maya archaeology. As a result of the superior quality of all of his work, Ruppert will be remembered forever as one of America's most distinguished archaeologists. In 1956, he resigned from the Carnegie Institution, and moved to Santa Fe. He holds the distinction of having the longest service record of any member of the Carnegie Institution's Division of Historical Research, which later became the Department of Archaeology.

His name has become synonymous with the work at Chichén Itzá, and his first field work for Carnegie was there. At this time, Sylvanus G. Morley was in general charge; Ruppert was an assistant to Earl Morris, on the excavation and repair of the Temple of the Warriors. He then went on to excavate, stabilize, and publish classic works on the Temple of the Wall Panels, Red House, Mercado, Sweat House, some of the ball courts, and numerous minor structures. Perhaps he is best-known for a major work at Chichén Itzá—the excavation and repair of the Caracol. His magnificent publication on this building is a masterpiece of meticulous recording, embracing excavation, architectural reconstruction, with drawings and photographs, and presentation of data.

From the time of his early field work and surveying in the Southwest, Ruppert continued to seek out the unknown or undescribed site. The hidden ruins in the jungle around Chichén Itzá came under his intensive scrutiny, whenever there was any spare time. He is credited with having summarized the architecture of Chichén Itzá, including an excellent classification of the individual buildings there.

Ruppert also will be remembered as an explorer, and as an excellent expedition man. With Morley, he participated in the exploration and study of Maya cities in the southern area, including Ezná and Calakmul in Campeche, and Yaxchilán on the Rio Usumacinta. Other explorations and research took him to the east coast of Yucatán, the interior of Campeche, Nicaragua, and elsewhere in Mesoamerica. When the Bonampak temple, with its magnificent wall paintings, was discovered, Karl Ruppert led the expedition to this jungle site where Antonio Tejeda, then with Carnegie, was to make copies of the paintings.

Ruppert has been described as a man "hard to know." Very few of those who, at first meeting, felt his so-called diffidence, realized how shy he was. He loathed large social gatherings of the cocktail party variety. He took to retirement far more gracefully than anyone I have ever known, but never lost interest in his first, enduring love—American archaeology. A little house, new, but not quite finished, on some of Santa Fe's most beautiful land, had been given to him by a devoted friend. This was the first home of his own he had ever known; he loved it dearly. It is sad that he had so few years in which to enjoy it, but during this Santa Fe period, he was extremely happy. Ruppert, the architect and homemaker, spent a good part of his time finishing the house, doing all the work himself. It contained his dearest personal possessions, and his library. The fortunate few, whom he numbered among his friends, enjoyed evenings full of pleasant company and good conversation, for he was a delightful host and an excellent cook. Taking little or no credit for himself, he was always eager to point out the good work of others, even to the extent of crediting them with some of his own accomplishments.

Karl was also a delightfully witty storyteller, although only a few friends knew this. Several of his anecdotes might well be included with the classic recitals of the professional bards, such as Kidder, Hodge, Morley, Nusbaum, and Chapman.

Those of us who came to know this quiet, shy, kindly, and, above all, scholarly man shall miss him very much. He gave his friendship to few, but it was both generous and genuine.

MARJORIE F. LAMBERT

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FACTS AND COMMENTS

RADIOCARBON DATES FOR SOUTHEASTERN FIBER-TEMPERED POTTERY*

RIPLEY P. BULLEN

ABSTRACT

Five new radiocarbon dates from an Archaic midden at the Palmer site on the Florida Gulf coast confirm the previous estimate of 2000 B.C. for the beginning of pottery making in the Southeast. Correlations over hundreds of miles give extremely close results between archaeological subperiods and radiocarbon dates.

CURRENTLY there is considerable interest in extremely early ceramic dates for the Americas. One early center for the manufacture of pottery was the southeastern part of the United States where the earliest pottery was tempered with fibrous materials. The recent receipt of five, as yet unpublished, radiocarbon dates for one stratigraphic column in an Archaic midden extending from the preceramic into the fiber-tempered period makes this a good time to review pertinent radiocarbon dates.

The accompanying chronological chart (Fig. 1) has been prepared to present the over-all picture. The five new dates, which are from the Palmer site on the Gulf coast of Florida at Osprey, a short distance south of Sarasota, have been arranged along the left side of the chart with their depths in the midden indicated. Data on these five dates are as follows: 0–1 feet, 1390 B.C. (G–596, 3350 ± 120 years B.P., 1960); 2–2.5 feet, 1265 B.C. (G–597, 3225 ± 120 years B.P., 1960); 4 feet, 1615 B.C. (G–598, 3575 ± 120 years B.P., 1960); 8 feet, 2090 B.C. (G–599, 4050 ± 125 years B.P., 1960); and 11 feet, 2140 B.C. (G–600, 4100 ± 125 years B.P., 1960); all from a letter of July 26, 1960, from H. N. Fisk of Humble Oil and Refining Company to Donn S. Gorseline of the Oceanographic Institute, Florida State University.

Similar data for the other dates include: Site 9HL60, 150 B.C. (C–933, 2104±140 years B.P., 1954, Libby 1955: 111); Zone 9 at Site J–5, 1195 B.C. (M–394, 3150±250 years B.P., 1955, Crane and Griffin 1858a); Refuge, 960 B.C. (M–267, 2920±200 years B.P., 1958, Crane and Griffin 1958b); Cotten, 1065 B.C. (M–215, 3020 ±200 years B.P., 1957, Crane and Griffin 1958b); Bilbo, upper, 3540 B.C. (0–1046, 5500±115 years B.P., 1960); Bilbo, lower, 2165 B.C. (0–1047, 4125 ±115 years B.P., 1960 — both Bilbo telegram of Feb. 3, 1961 from H. N. Fisk, Humble Oil and Refining Co.); Dulany, 1810 B.C. (M–236, 3770 ±200 B.P., 1958, Crane and Griffin 1958b); and Sapelo, 1750 B.C. (M–39, 3700 ±250 years B.P., 1952, Crane 1956). Reference will also be made to a date from the Hunter site at Red Lake in New York State of 2450 B.C. (C–794, 4400±260 years B.P., 1953, Libby 1954: 137).

The stratigraphic situation at the Palmer Archaic midden, a plain (undecorated) fiber-tempered zone below one in which the pottery was decorated and above one which contained no pottery, is the same as that found at the excavated portion of the Bluffton site, on the St. Johns River 150 miles to the northeast (Bullen 1955). The type of decoration on Orange Incised sherds from the upper levels of the Palmer section is similar to that from comparable levels of the Bluffton site and different from that at the South Indian Field (Ferguson 1951), Cotten (Griffin and Smith 1954), and Summer Haven (Bullen 1961) sites.

These data confirm temporal divisions of the Orange period of Florida presented elsewhere (Bullen 1954). The South Indian Field, Cotten, and Summer Haven sites represent a late, while the upper levels at Bluffton and Palmer represent an early, phase of the decorated portion of the Orange period. In Georgia plain and subsequent decorated fiber-tempered phases are recognized but the decorated phase has not been subdivided.

The chart, except for the upper Bilbo date (to be mentioned later), presents an amazing picture of radiocarbon dating consistency. The minor irregularity between the dates for the 0–1 and the 2–2.5 foot levels at the Palmer site may be disregarded. It is probably the result of local disturbance, for we found disturbed burials at a depth of 1 to 2 feet.

The neatness with which the Georgia plain fiber-tempered dates from Sapelo and Dulany fit between the Palmer dates for the beginning and end of that period is most gratifying. These two Georgia dates came from the mid-portion of such deposits and were not intended to indicate the earliest pottery (A. J. Waring, Jr., personal communication). The lower Bilbo date, while probably a little too early, is certainly of the correct order of magnitude and compares favorably with the two lowest Palmer dates. Similarly the Cotten site date for the late phase of the decorated portion of the Orange period is satisfyingly more recent than the Palmer dates for the early decorated phase.

Several limiting or bracketing dates have been included in the chart. The 2140 B.C. date for a depth of 11 feet at the Palmer site, 3 feet below the deepest sherd, needs no further comment. The Refuge site is believed to reflect the earliest Woodland influences at the mouth of the Savannah River in South Carolina as evidenced by a change from fiber-tempered to sand-tempered vessels (A. J. Waring, Jr., personal communication). The date, 960 B.C., agrees nicely with the other dates. Similarly, we have indicated the 150 B.C. date for Site 9HL64, a late Deptford period site (Fairbanks 1954).

The only divergent date, still omitting the upper Bilbo date, is the one shown for a deep component of Site J–5, located on the Chattahoochee River in northwestern Florida. This component must be early Transitional in date because it contained St. Johns Incised and St. Johns Plain sherds (stratigraphically post-fiber-tempered at other sites) as well as Orange Plain (fiber-tempered) sherds. It was also stratigraphically below a Deptford component (Bullen 1958b). The date shown, 1195 B.C., is surely too early by several hundred years.

* Presented at the 26th Annual Meeting of the Society for American Archaeology, May 4, 1961, Columbus, Ohio.

This discrepancy calls for a comment. The Florida Cotten and Palmer dates are based on runs made on marine shells taken from middens composed, chiefly, of other marine shells. Two of the Georgia plain-tempered dates, Sapelo and Dulany, are based on oyster shells, but they have been checked by a run on shells of oysters currently living in coastal Georgia (A. J. Waring, Jr., personal communication). The Refuge date is also a shell date, from a depth of 3 feet in an 8-foot shell midden (Crane and Griffin 1958b: 6). There is little reason to anticipate any radiocarbon contamination of any of these samples, and the internal consistency of the dates from them is certainly excellent.

The other dates, from 9HL64, J–5, and Bilbo, are all charcoal dates. Material for the J–5 date consisted of charcoal taken from a 6-inch thick occupation zone buried 8 feet deep in a natural levee. The cultural deposit included a small fireplace, fresh-water mussel shells, sherds, and stone tools (Bullen 1958b). The Chattahoochee River flows through limestone country and floods the J–5 site every year. While charcoal is not supposed to become contaminated by antique $CaCO_3$, it should be noted that any effect from the associated mussel shells, the surrounding dirt, or percolating water would tend to make this date older than otherwise. Also, possibly, some antique carbonate may be absorbed by trees growing in limestone country through their roots.

The Bilbo site consisted of a deep midden in which a decorated fiber-tempered pottery zone overlay one in which the pottery was undecorated. The Bilbo upper date, from a depth of 2–2.5 feet, came from the decorated zone while the lower date, from a depth of 5.5–6 feet, belongs to the plain pottery horizon (A. J. Waring, Jr., personal communication). The Bilbo upper date is extraordinarily early, contrary to all the other dates, and obviously incorrect. It should be disregarded.

As suggested earlier, the Bilbo lower date, when compared with the shell dates, seems a little too early by a couple of hundred years. This "amount of earliness" is similar to that mentioned for the date from Zone 9 at Site J–5, another charcoal date. This similarity suggests a similar factor may be the cause. If these two charcoal dates are to be considered more reliable than the marine shell dates, a proposition with which I disagree, then about 200 years should be added to the shell dates. That would tend to make the southeastern fiber-tempered

FLORIDA					GEORGIA	
Radiocarbon Date B.C.	Site	Period			Site	Radiocarbon Date B.C.
		DEPTFORD			9 HL 64	150
1195	J-5, Zone 9	TRANSITIONAL			Refuge	960
1065	Cotten	Late Decorated	FIBER-TEMPERED POTTERY	Decorated	Bilbo	(3540?)
1390 1265	Palmer 0-1 ft. 2-2.5 ft.	Early Decorated				
1615	4 ft.	Plain		Plain	Sapalo Dulany Bilbo	1750 1810 2165
2090	8 ft.					
2140	11 ft.	PRECERAMIC				

FIG. 1 [Bullen]. Comparative chart of radiocarbon dates for fiber-tempered pottery horizons in Florida and Georgia.

ceramic period a little earlier than this article indicates.

Radiocarbon dates show the fiber-tempered period of Southeastern United States to have started in Florida and Georgia by 2000 B.C. and to have lasted until about 1000 B.C. In bóth areas a decorated period follows a plain period while in Florida the decorated period has been divided into an early and a late phase. Radiocarbon dates now available agree in sequential arrangement with the archaeological divisions previously defined.

There are several radiocarbon dates which indicate Woodland pottery was well established in north central and northeastern United States around 1000 B.C., at the close of the fiber-tempered ceramic period in the southeast, and one extremely early, divergent date of 2445 ± 260 B.C. This divergent date comes from a radiocarbon run on charcoal found at a depth of 25–30 inches in a burial pit at the Hunter site on Red Lake in New York State (Ritchie 1955: 66). There was no pottery in this or any of the other excavated burial pits at this site. The allocation of this early date to a ceramic period is based on the finding of Vinette I sherds, the earliest type in the region, on the eroded surface of the Red Lake site in three small concentrations (Ritchie 1955: 25–6). The temporal association of these sherds from the eroded surface with charcoal in a pit, whose top had been cut off at a depth of 10 inches by plowing (Ritchie 1955: 27), is questionable. I believe this date should not be considered applicable to the earliest pottery in northeastern America because: (1) the association is extremely tenuous; (2) it would mean that one pottery type, Vinette I, lasted for nearly 1500 years before the development or introduction of Vinette II types; (3) it is contrary to several dates suggesting 1000 B.C. to be correct for the first Woodland pottery; (4) it violates five dates from Long Island which indicate the preceramic occupation was still present 1000 to 750 B.C. (Ritchie 1959: 75).

Elsewhere I have presented the arguments for an independent invention of pottery in Florida and Georgia (Bullen 1959, 1960). It suffices to say here that archaeological correlations, lack of typological similarities, and radiocarbon dates all combine to document the fiber-tempered pottery of Florida and Georgia as the earliest pottery north of Mexico.

Since writing the above, we have received through the courtesy of James B. Griffin another radiocarbon date pertaining to the Florida fiber-tempered period. The sample consisted of ocean shells and came from the 18–24 inch level of Square B at the Summer Haven site (Bullen 1961). The date is 1369 B.C. (M–1014, 3330± 200 years). The determination was made at the University of Michigan Memorial–Phoenix Project Laboratory. Archaeologically this sample belongs to the late decorated part of the Florida fiber-tempered period as it was associated with steatite sherds and fiber-tempered pottery having wide decorated rims. The date while a little earlier than anticipated agrees well with the other dates presented on the accompanying chart.

Acknowledgments. We wish to thank the Humble Oil and Refining Company for making the radiocarbon determinations on the Palmer samples, H. N. Fisk of that company for his courtesies in connection with the Bilbo site dates, Charles H. Fairbanks of Florida State University for arranging for the dating of the Palmer samples, Antonio J. Waring, Jr., of Savannah for making the Bilbo dates available for this article, and James B. Griffin and the University of Michigan for the Summer Haven date.

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Florida State Museum
Gainesville, Fla.
January, 1961

A BRIEF PROGRESS REPORT ON THE EXCAVATIONS AT CERRO PORTEZUELO, VALLEY OF MEXICO

H. B. Nicholson and Frederic Hicks

Abstract

Work at this Classic-Postclassic site, begun by George W. Brainerd and interrupted by his untimely death, is being completed by Nicholson and Hicks. While analysis of much of the archaeological material is well underway,

work on the ceramic typology has not yet proceeded to the point where "functional" interpretations can be made. Certain misunderstandings in the use of Cerro Portezuelo material by Mayer-Oakes are noted.

A REPORT on the excavations at Cerro Portezuelo, Mexico, initiated in 1954 by the late George Brainerd, has been long delayed, largely because of his sudden, premature death in 1956. A number of Mesoamerican archaeologists have expressed interest in the progress of the project. We take this opportunity to summarize concisely its present status. Nicholson presented a brief oral summary during the symposium on the archaeology of the Valley of Mexico at the 58th Annual Meeting of the American Anthropological Association, Mexico, December 29, 1959. However, work has advanced considerably since that time, and a more up-to-date statement may be of interest.

The site is located in the southeastern Valley of Mexico, strung out along the north flank of the northernmost of a series of east-west trending ridges which at the time of the Conquest divided the province of Acolhuacán from Chalco. It was selected by Brainerd at the suggestion of Paul Tolstoy, who had surface-collected it, because it promised to provide significant information concerning the Classic-Postclassic transition. With financial support from the Wenner-Gren Foundation for Anthropological Research and UCLA, Brainerd excavated at Cerro Portezuelo during two successive summers. The 1954 season was devoted primarily to widespread stratigraphic test-pitting, the exploration of the principal ceremonial complex, and mapping. The 1955 season saw a continuation of these projects, but principal attention was directed to architectural investigations in two areas only preliminarily tested the season before. Following the first season, Brainerd initiated his analysis of the ceramics. He worked out over a hundred preliminary sorting categories, whose numerical frequency in the retained sherd material was tabulated according to pit and level by his assistants. These sorting categories were only tentative, and their tabulation was preliminary to a statistical analysis directed to discovering cluster patterns which, in turn, were to lead ultimately to the final formulation of types. Brainerd apparently consciously intended to ignore all previous Valley of Mexico ceramic typologies while constructing his own, although he undoubtedly planned on comparing his final results with those of other workers.

Following the 1955 season, as has been recounted by Mayer-Oakes (1959: 356), Brainerd presented a report on his Cerro Portezuelo work to an interested group of archaeologists during the 54th Annual Meeting of the American Anthropological Association in Boston, at the Peabody Museum, Cambridge, November 16, 1955, during which he showed a number of color slides of whole vessels from the cache lots. The tabulation of the sherd material recovered during the second season was in progress when Brainerd suddenly died, February 14, 1956. His only published statement on his Cerro Portezuelo work appeared posthumously (Proskouriakoff 1956: 441). At this time, aside from the unfinished tabulation of the preliminary sorting categories, only the cartographic work for the 1954 season and some odds and ends of photography and drawing had been completed.

In the fall of 1956, Nicholson, who had replaced Brainerd at UCLA, assumed the responsibility of completing the project. To familiarize himself at first hand with the site, he conducted brief excavations adjoining one of Brainerd's principal excavated areas in the summer of 1957 (Proskouriakoff 1958: 225). In the summer of 1958, he test-pitted a site in the nearby town of Chimalhuacán, for comparative purposes. The sherd material recovered during these two brief excavations has been preliminarily sorted, pending the completion of the final Cerro Portezuelo typology. In 1960 Hicks, a field assistant during the 1955 season, entered into full collaboration on the project. To date, the transcription and organization of the field notes kept by Brainerd and his various assistants has been accomplished, site maps and architectural plans have been nearly completed, photography and description of cache lots and principal artifacts is well underway, and work on the ceramic typology has been initiated.

Of crucial importance to the chronological interpretation of the site — and to the Valley of Mexico ceramic sequence in general — are a series of burial and cache lots containing many complete or nearly complete pottery vessels, a number of which were discussed and illustrated by Brainerd during his Cambridge talk of November 16, 1955. These clearly span both the Classic and Postclassic periods. Although their analysis is incomplete, certain "anomalous" associations are evident, which may necessitate some revisions in the dating of the appearance of various Valley of Mexico ceramic types. Most of the Teotihuacán style vessels were preliminarily typed in 1955 by Armillas, at Brainerd's request, in terms of his Teotihuacán typology. The cache lots were also recently inspected by Millon and Bennyhoff; it is hoped that they can be supplied soon with data to aid their analysis of their Teotihuacán materials.

We should also like to take this opportunity to make two brief observations concerning Mayer-Oakes' use in his El Risco report of the Cerro Portezuelo materials he inspected in December, 1956 and January, 1957. Mayer-Oakes (1959, Table 9) published simplified versions of Brainerd's preliminary sorting category tabulation charts for seven of the Cerro Portezuelo pits. Due to the courtesy of Mayer-Oakes, we had the opportunity of seeing the El Risco manuscript before its publication. It did not, however, contain these detailed charts themselves, which were only briefly described in the text. No explicit authorization for their publication was sought or given, and it is unlikely that Brainerd ever intended their publication. They constituted mere preliminary, highly tentative tools used in work toward the final formulation of types. No valid typological conclusions can be based on them alone. In our final report, Cerro Portezuelo will be compared ceramically with El Risco, along with other important Valley of Mexico sites, and

Mayer-Oakes' suggestions as to their relationship will be carefully examined.

One serious error in the El Risco report, caused by an unfortunate misunderstanding, stands in need of prompt correction. This was attempted by Nicholson orally before publication, but it was apparently too late. Mayer-Oakes (1959: 356–7) described the "synoptic Portesuelo exhibits Brainerd had arranged" at UCLA and cited them as evidence for Brainerd's periodization of the site. Actually, this exhibit, as was explained by Nicholson to Mayer-Oakes at the time the latter inspected it, was hastily improvised in time for the 1956 Santa Monica meeting of the American Anthropological Association by Nicholson, months after Brainerd's death, and included materials from more than one collection. It in no way reflects either Brainerd's or our interpretation of the Cerro Portezuelo sequence.

With regard to the "functional" interpretation of Cerro Portezuelo by Mayer-Oakes (1959, 1960), and comments thereupon by Millon (1960), we prefer not to express our opinions at this time. The analysis of the Cerro Portezuelo ceramics and architecture has not proceeded to the point where firm conclusions along this line can be drawn. In the final report, this problem will be explored at some length, at which time the suggestions of other investigators will be thoroughly discussed.

If the necessary financial support can be obtained, it is hoped that the long-delayed Cerro Portezeulo excavation report can be completed within the next two years. Meanwhile, we ask the patient indulgence of our coworkers in Valley of Mexico archaeology. Owing to the premature death of the able student who conceived and guided the project in its initial stages, the task has been difficult and slow. Our report will unavoidably be somewhat different from that which would have resulted had the project's creator lived to complete it. We intend, nevertheless, to present the fullest possible record of the excavation of this important Valley of Mexico site, which should add significantly to our knowledge of the preconquest culture history of this key area of aboriginal America.

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UNIVERSITY OF CALIFORNIA
Los Angeles, Calif.
February, 1961

A HOPEWELL HUMAN BONE WHISTLE

RAYMOND S. BABY

ABSTRACT

An engraved human bone whistle, from a burial mound near Bourneville, Ross County, Ohio, described for the first time as part of the Hopewellian complex, was probably used in elaborate burial ceremonies.

IN THE SPRING of 1959, grading, widening and resurfacing of U.S. Route 50, just east of Bourneville, Twin Township, Ross County, Ohio, revealed eight prehistoric Indian burials along the north edge of the road bed. These burials were beneath a small remnant of the Bourneville Mound (McBeth 1960), which was part of a Hopewellian mound complex known as the Unnamed Works. The mound group consisted of large and small circular earthen enclosures, and two subconical burial mounds (Squier and Davis 1848, Pl. 30, No. 3). The smaller circular enclosure and the Bourneville Mound were destroyed during the original road construction; disposition of the second mound in the group is not known.

In spite of the extensive damage done to the site and the remains that it covered, some data were obtained from the workmen. All of the burials were extended inhumations made on the floor of the mound and covered with earth and slabs of sandstone.

Most of the artifacts — consisting of a copper breast plate, four copper earspools, ten fresh-water pearl beads, two worked grizzly bear canine teeth, and two worked and engraved pieces of bone — were recovered and brought to the Ohio State Museum by David Medert, a State Highway Patrolman. These objects were reported to have been associated with one of the eight burials.

Cleaning and restoration of the two worked and engraved pieces of bone revealed a whistle fashioned from a right human radius. The morphological characteristics and the metric dimensions (maximum length of 209.5 mm.) suggest that the bone can be assigned to a female individual. Union of the proximal or upper epiphysis, and slight lipping and eburnation of the lower margin of the head places the age between 40 and 45 years — the onset of the aging period.

The bone shows marked signs of careful preparation such as cutting, scraping, polishing, and drilled perforations. (Fig. 1). The distal or lower head had been removed and bound with a thin (20 mm. wide) copper band, 12 mm. above the cut margin. Copper stain about the neck of the bone indicates the existence of a second but narrower band, approximately 13 mm. wide. Unfortunately, it was lost at the time of discovery. A triangular opening, measuring 12 mm. high and 8.5 mm. in width, is cut in the lower posterior surface, just above the copper band. Lateral to the triangular opening are two elliptical perforations, 3.5 by 6.5 mm.; and 27 mm. below the radial tuberosity are two small drilled perforations, 3.5 mm. in diameter. The latter were probably used for suspension.

A geometric design is engraved along the entire posterior surface of the bone between the copper band and the tuberosity (Fig. 2). The design elements, consisting of cross-hatched lobes, tear-shaped forms and circles, are symmetrically arranged and attached to curvilinear lines, some of which assume the shape of the cross-hatched forms. The elements are conventionalized forms common to Hopewell.

Worked and engraved sections of human long bones, "trophy" skulls, worked skull and jaw parts, and the use of a human mask-headdress are well known for the Hopewell culture. This whistle fashioned from a human long bone is unique and has not been previously reported. It undoubtly was a musical instrument and part of the paraphernalia used in connection with the elaborate burial ceremonies and customs of the Hopewellian people.

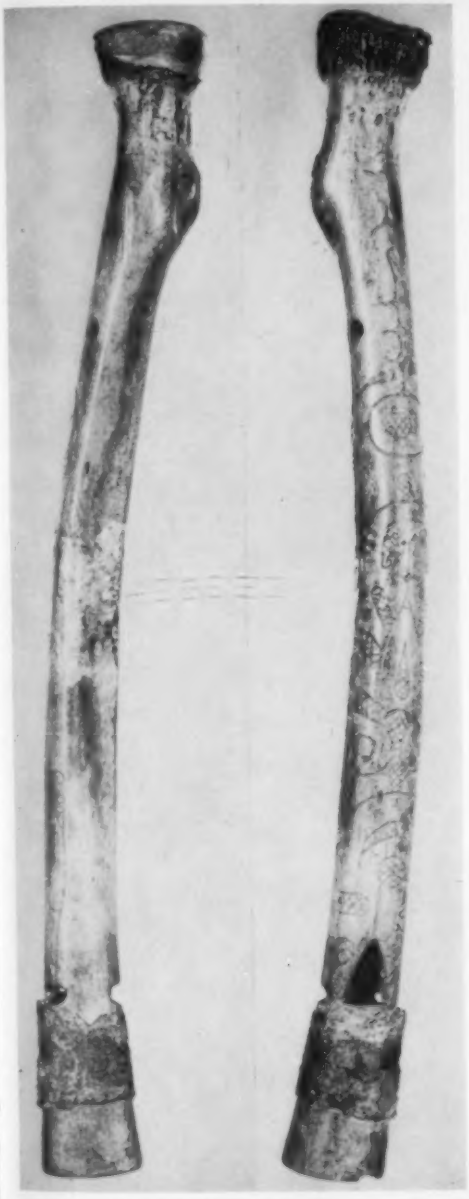

FIG. 1 [Baby]. Engraved bone whistle made from right human radius, Bourneville Mound, Ohio Hopewell.

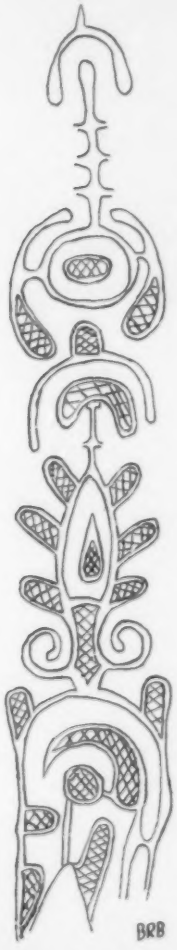

FIG. 2 [Baby]. Drawing by Bruce R. Baby of the engraved design on the Hopewell human bone whistle from the Bourneville Mound, Ohio.

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Ohio State Museum
Columbus, Ohio
February, 1961

FIXING SITE LOCATIONS BY RADIO-DIRECTION FINDER AT MESA VERDE

Alden Hayes and Douglas Osborne

Abstract

The development and use of a radio-direction finder system for site location in areas where other methods may fail has been successfully accomplished by the National Park Service's Wetherill Mesa Archeological Project. A brief, non-technical statement concerning the equipment and its use is given.

THE ARCHAEOLOGICAL survey of Wetherill Mesa, Mesa Verde National Park, was greatly aided by the use of radio-direction finding equipment in triangulation. The comparatively featureless and heavily wooded mesa top does not lend itself to rapid or accurate location by means of chaining or pacing to points sighted by Brunton compass. The 225 days involving the use of the RDF on the Wetherill Mesa survey would probably have been doubled and likely done with less accuracy by the use of conventional methods. An estimated saving of between $3500 and $4000 was made.

The two radio transmitters were set up over known points and set to emit a steady signal at different frequencies. A portable receiver, carried to the archaeological site, was mounted on an azimuth base and tuned to each frequency in turn. When directly "on course" an audial null marks the bearing on the transmitting station. The azimuth was later plotted on the map by simple triangulation.

During the planning stage of the Wetherill Mesa Archeological Project in 1958, Carroll A. Burroughs, then Park Archeologist, Mesa Verde, conceived of the method and worked out the requirements of specifications of the beacons with the cooperation of the Service's Western Office of Design and Construction. The origin and early successful development of the system is due largely to Burrough's imagination and persistence.

The two transmitters, Type 5904, developed and manufactured for us by Learned, DePuy, and Johnson, 127 Montgomery Street, San Francisco, are self-contained, portable units designed to mount on a surveyor's tripod (Fig. 1). Each was fixed by the manufacturer to the frequencies assigned by the Federal Communications Commission, in our case 2770 KC and 3237 KC, and were designed to produce a signal readable at one-half mile. Each transmitter with antenna weighs 5¾ pounds. The equipment is readily portable for short distances but when some distance or rough terrain is to be traversed it can be packed with sponge rubber in heavy cartons and strapped to pack-boards. We soon found that the self-contained dry cell batteries did not have enough power to send a clear signal much over 500 yards. A booster pack made up in a .30 caliber ammunition box by a local radio man raised the weight of each beacon unit by 9¾ pounds but also raised transmitter output to the desired range (Fig. 2).

The use of low-powered beacons depends on the availibility of numerous known points. We are fortunate at Mesa Verde in having a fairly accurate topographic map. The many small rincons along the cliff edges and several cliff dwellings indicated on the sheet offer known spots for the location of reference points. In addition a traverse was run by transit for most of the length of the

Fig. 1 [Hayes and Osborne]. Transmitter on surveyor's tripod.

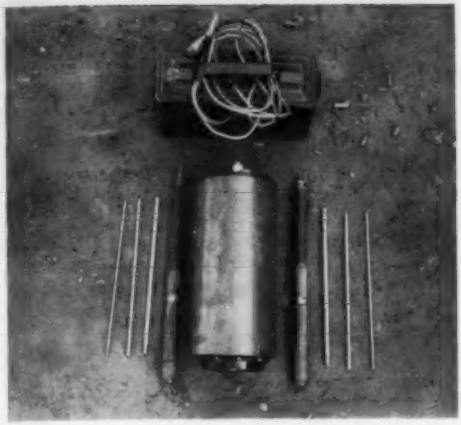

FIG. 2 [Hayes and Osborne]. Component parts of transmitter with booster pack.

mesa and 21 ARP's (archaeological reference points) were established and marked by a brass cap in a small concrete block.

The receiver used is a Roco Radio Compass, Model 303NML, manufactured by Robinson and Company, 731 West 129th Street, Gardena, California. The set, measuring 6 by 8 by 7 inches and weighing about seven pounds with batteries, is easily hand-carried by the ferrite loop antenna which also serves as a handle. The receiver is used with an azimuth base, a board equipped with a movable compass rose and a centered, metal frame or carrier for the receiver. The board is placed on level ground and made secure from accidental movement and the compass rose oriented to true north by means of a Brunton (Fig. 3). The set is placed in the carrier, tuned to the proper frequency and rotated until the audial signal is faint or nulled completely. The area of null will cover a spread of from 1 to 25° so the bearing should be read from both directions; the set is rotated clockwise until the null is reached, the bearing read, the set then turned counter-clockwise until a null is reached. The difference between the two readings is then divided to get a mean reading.

In experimenting with the equipment prior to actual surveying, the transmitters were placed at known points previously located by transit and the receiver taken to a third known spot. Repeated readings using varying combinations of the three points gave consistent readings with not more than a two degree error. At 2000 feet this will locate a point within 65 feet — accurate enough for our purpose.

It is recommended that new operators practice with the receiver using a similar group of three known reference points. The visual direction indicator on top of the receiver cabinet is not sensitive enough to be activated by the low-powered transmitters. Aural recognition of the null is necessary. The operator's ear must be trained to recognition of variation in sound and to intense concentration on the job at hand. The loudspeaker is usually used but on windy days a set of headphones is plugged into the jack provided to eliminate local noise. With a few hours training a man can learn to orient his compass rose and then can obtain the two necessary azimuths in from ten to fifteen minutes.

Employing the RDF equipment a three man crew on Wetherill Mesa's heavily wooded surface recorded an average of from six to eight sites a day. One man secured the bearings while another made the field notes and sketches and a third made a surface collection. The duties were rotated occasionally to relieve the strain of concentration on the radio operator.

The system is not a foolproof, down-hill pull. Though designed for portability the receiver and particularly the beacons are delicate instruments and must be treated gently to avoid loss of calibration. When transported over rough roads in the back of a pickup they should be

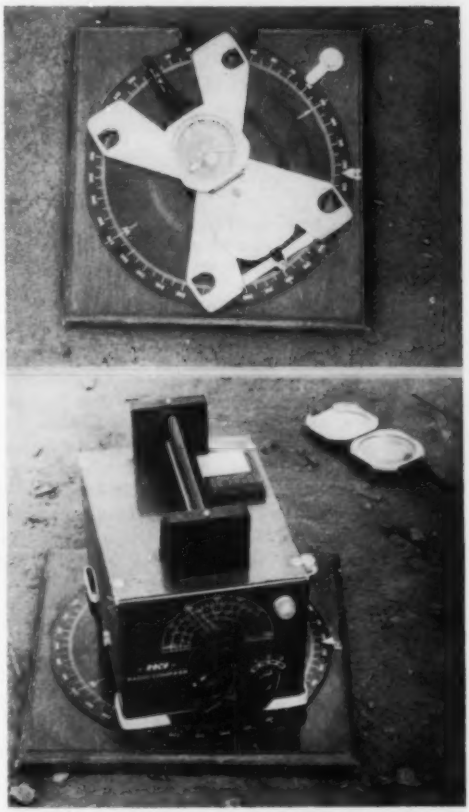

FIG. 3 [Hayes and Osborne]. Azimuth board and receiver. *Top*, orienting compass rose on azimuth board with Brunton compass; *bottom*, receiver in position on azimuth board.

carefully packed; a thing we learned the hard way. Another note of warning concerns the operator of the receiver. When the set is rotated to approach the null from the two directions, care must be taken not to vary the position of the hands of the receiver. When the handle containing the ferrite rod antenna is touched the signal will be louder than if the set is turned by the carrier on the azimuth board. A change in intensity of sound from one reading to the next from the same transmitter could result in an error of several degrees.

Generalized technical plans, instructions, and other details are available at Mesa Verde National Park. We shall be glad to send copies of them to interested colleagues, and to answer questions and queries.

Wetherill Mesa Archeological Project
Mesa Verde National Park, Colo.
February, 1961

EARLY POINT TYPES FROM ACASTA LAKE, NORTHWEST TERRITORIES, CANADA

Richard G. Forbis

Abstract

A small collection of stone artifacts from an esker-ridge site at the edge of the Barren Lands, which includes an Agate Basin point and two Lerma points, but apparently no microblades, may be the earliest so far recognized in the Canadian tundra.

IN OCTOBER, 1960, Ernest Reinhold of the University of Alberta in Edmonton, forwarded 21 stone specimens to the Glenbow Foundation for examination. They had been found by C. V. Sullivan and T. E. Vaasjo, of Associated Helicopters, 80 miles southwest of Port Radium, on Great Bear Lake, Northwest Territories, Canada.

The locus of the discovery, at the edge of the Barren Lands, was the east end of an esker ridge running through Acasta Lake (65° 24′ N. Lat.; 115° 31′ W. Long.). The helicopter crew used the location as a base camp, and, upon finding evidence of prehistoric occupation in caribou trails and blowouts, they excavated two pits covering 460 square feet. The pits centered about a large quartzite boulder, known as the Supply Rock, which had been the source for stone for most artifacts. Below 4 inches of sterile leaf mold, they came into a bed of sand of undetermined depth, the top of which was thick with workshop debris. Artifacts were found in the top 12 inches of the sand; microblades, if present, were overlooked by the discoverers. Hearths, outlined by granite rocks about the size of a man's head, were about 18 inches across and 10 inches deep. No charcoal was saved; bones were absent.

Of three projectile points, one is a slim, well-made point of light-colored vein quartz (Fig. 1 *a*). Essentially lanceolate, it nevertheless has slight shoulders about one-third of the way from the base. It is 8 mm. thick.

Fig. 1 [Forbis]. Projectile points (*a-c*) and knife (*d*) from Acasta Lake, Northwest Territories, Canada.

In appearance, it is reminiscent of some early point types from the Great Plains. Comparable specimens are reported from other locations in the Northwest Territories, at Barlow and Grant Lake (Harp 1959, Figs. 2 *r*, 3 *ff*). It may be significant that these sites, too, are on esker tops.

The second specimen is lanceolate and pointed at both ends (Fig. 1 *b*). It is a typical Lerma point. Made of dark porphyritic basalt, it is almost identical in size to the aforementioned specimen except in thickness (12 mm.). The point is strongly beveled along one edge near the tip. As with the first point, over-all flaking tends to be irregular and resolved. The base is not ground.

The last projectile point and all remaining specimens are made of quartzite (Fig. 1 *c*). It too appears to be a Lerma point, even though the unground base tends to be squared rather than pointed. Cruder than the others, it is 15 mm. thick. Borden has found similar specimens at an early site on the Fraser River, near Yale, British Columbia. At this site, DjRi–1, they are found in a layer dated by radiocarbon at 8150 years ago; they may also characterize an earlier layer estimated to be about 9000 years old.

An artifact of possible, but undetermined, diagnostic value is a long narrow knife, bifacially chipped to varying degrees of fineness (Fig. 1 *d*). Aside from being rounded at both ends, it bears a generic resemblance to one specimen from DjRi–1. The remaining complete artifacts include a small, leaf-shaped scraper, strongly plano-convex in cross section; and a large, thick chopping tool, bifacially chipped and pointed at one end. Fragmentary specimens consist of tips and bases of large ovoid and pointed bifaces. Such tools have no diagnostic value, being widespread both geographically and temporally. They are much like the "large blades" illustrated by MacNeish for his Lockhart River complex (1951, Pl. 5).

The first projectile point from Acasta Lake (Fig. 1 *a*) clearly falls into MacNeish's "Agate Basin or Yuma Tradition," comprising such complexes as Champagne, Artillery Lake, Great Bear River, and Flint Creek (MacNeish

1960: 48). All these complexes are thought to have been derived from earlier horizons of the Great Plains, perhaps no more than 4000 to 7000 years ago, before the Northwest Interior Microblade Tradition entered from Asia.

It is most unusual to find Lerma points in such a setting as Acasta Lake, for, as MacNeish has pointed out, they seem to belong to an intermontane or cordilleran tradition. The specimens from Acasta Lake are the only ones yet reported from the Canadian tundra; the closest comparable specimens seem to be from Firth River, Kluane Lake, and central British Columbia. Lerma points in British Columbia are not necessarily early. They may have persisted for a long time, perhaps from 9000 to 2000 years ago, according to Borden. This estimate is in accord with that of Suhm and Krieger (1954: 440), who say that Lerma points possibly "appeared . . . several thousand years before Christian era; in Texas from well before the time of Christ to somewhat after."

The Lerma points from Acasta Lake, therefore, may not be ancient. Yet their context — with a point in the Agate Basin or Yuma tradition, and in a site apparently lacking the ubiquitous microblades of middle periods of the Northwest Interior — suggests that the Acasta Lake finds are among the earliest so far recognized in the area. MacNeish has written me that the "Acasta Lake find could be a case in which the southward-spreading Lerma type has lasted up until it met the northward-moving Agate Basin types."

Acknowledgments. I am grateful to T. E. Vaasjo and C. V. Sullivan for their cooperation; and to C. E. Borden and R. S. MacNeish for their comments which, when not documented, were verbally given.

HARP, E. J.
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GLENBOW FOUNDATION
Calgary, Alberta
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IN FAVOR OF SIMPLE TYPOLOGY

JAMES A. FORD

ABSTRACT

Proponents of the "type-variety" method of ceramic analysis are criticized for mixing analytical units and the rank hierarchies appropriate to synthesis, and for semantically substituting "type-variety" for "type" without actually changing the method of analysis.

IN RECENT issues of AMERICAN ANTIQUITY, a number of pages has been devoted to the new "type-variety" concept and some archaeologists are using it in analyzing their materials. Wheat, Gifford, and Wasley (1958) expound the methodology clearly, and illustrate with Southwestern ceramics. Phillips (1958) has applied it to Eastern ceramics, while Smith, Willey, and Gifford (1960) have done the job for the Maya region. The discussion ends (for the present) with a theoretical exposition of the method by Gifford (1960).

Just to build up my bibliography, I should like to register a feeble word of protest. What is being done by these authors is excellent, but what they say about what they are doing is a little confusing. Archaeological research, like all research, consists of two balanced processes. *Analysis* is "separation of anything, whether an object of the senses or of the intellect, into constituent parts or elements," (Neilson 1961: 94). The archaeologist must divide or take apart his data into as small units as he either chooses or as is possible for purposes of examination and comparison. *Synthesis*, on the other hand, is " . . . a putting together . . . The combination of separate elements of thought or sensation into a whole, as of simple into complex conceptions, or species into general — the opposite of analysis. Hegel." (Neilson 1961: 2560).

Dendritic classifications modelled on the biological system of Linnaeus have long been used by culture historians, and with considerable reason, for cultures do tend to branch off from one another like species. The several Romance languages certainly came from the same source. Useful as dendritic systems are as framework or outline for presenting a history which is already known, they are not tools for the analytical phases of research.

When the Midwestern Taxinomic Method was proposed by McKern (1939) 32 years ago, he was careful to state that the system had no historical implications. However, history was just what was needed. The technique was misused by a number of students in an attempt to make it an analytical tool and so received much unjustified criticism and fell into disuse. Gladwin (1934) and his co-workers evolved and employed a similar system to present the outline of Southwestern prehistory. It was not used as a tool. Willey and Phillips (1958) have recently made proper use of a flexible dendritic framework.

Smith, Willey, and Gifford (1960: 335) clearly state:

> A sharp distinction is necessary between the basic working units of ceramic analysis on one hand and devices of a higher order of synthesis on the other. Among the latter may be included concepts such as "tradition," "ceramic system," [etc.]. . . . these units [types and type varieties] *must* be maintained as free agents of analysis.

Yet they go ahead and propose to mix analytical units and the rank hierarchies appropriate to synthesis. What is the type in their system but a higher order than "Type-Variety"? The nature of the proposed procedure is further brought out by these same writers (p. 333): "As a result types are actually best left undefined until the very end of the analysis." In the next paragraph: "In practice as well as in theory, then, when adhering to the type-variety concept as defined, all working ceramic analytical units are varieties from start to finish."

So, the "type-variety" replaces the old working unit we used to call "type," and "type" is now elevated to the

next higher level in the process of synthesis. Relieved from common drudgery, it has become an executive. It is hard to see what is new about this except the switch in terminology. I suspect that this move has been motivated by a feeling that there is in culture history a naturally packaged unit to which the name "type" belongs. In argument, I can only cite my earlier statements (Ford 1954).

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RADIOCARBON DATES FROM THE LODAISKA SITE, COLORADO

HENRY J. IRWIN AND CYNTHIA C. IRWIN

ABSTRACT

Seven radiocarbon dates place the regional sequence from 3000 B.C. to A.D. 1000, validate partially the method determining complexes at the site, and date the beginning of maize agriculture in the area.

RADIOCARBON age determinations on seven samples of charcoal from the LoDaisKa site have been obtained through the kindness of H. R. Crane and the University of Michigan Memorial–Phoenix Project Laboratory. These were submitted to J. B. Griffin of the University of Michigan. We are grateful to both Crane and Griffin for their help. The samples were collected with the usual precautions. For some of them it was necessary to make a preliminary concentration of charcoal by flotation using distilled water and metal screening. The samples were then sealed in Mason jars after baking in an oven to destroy bacteria.

The samples located stratigraphically in terms of inches below baseline are as follows: M–1002, A.D. 700 (1260±150 B.P., 40–50 inches); M–1003, A.D. 990 (970±150 B.P., 40–52 inches); M–1004, 1440 B.C. (3400±200 B.P., 62–64 inches); M–1005, A.D. 810 (1150±150 B.P., 58–60 inches); M–1006, 1190 B.C. (3150±200 B.P., 68–76 inches); M–1008, A.D. 810 (1150±150 B.P., 70–74 inches); M–1009, 2880 B.C. (4840±250 B.P., 90–94 inches).

These dates correlate stratigraphically with cultural complexes detailed in the site report (Irwin and Irwin 1959). These complexes were labeled A through D. Complex A was represented by a very few artifacts but included diagnostic types of pottery, projectile point, and gaming piece suggesting affinity with what has been called the Fremont culture in western Colorado and eastern Utah. The date of A.D. 990 (M–1003) may pertain to this occupation or a late phase of Complex B. Complex B represents an occupation of the widespread, and still little known, Plains Woodland culture. Pottery and projectile points are diagnostic. Certainly the dates A.D. 700 (M–1002) and A.D. 810 (M–1005) fit this complex both in terms of stratigraphy and chronology since a date of A.D. 800 (1150± 150 years) has been reported by Lamont Laboratory from a "Woodland" site in the Denver area (Hunt 1954: 114).

Complex C was recognized by the distribution of certain types of artifacts including distinctive projectile points, scrapers, and associated tools. Complex C was the most difficult to sort out. It clearly belongs to a group of Great Plains "hunting cultures" found elsewhere at Signal Butte in Nebraska and at the McKean site in Wyoming, which has given its name to this manifestation. However, overlap of this group with Complex B above, and Complex D below, made it very difficult to set stratigraphic or cultural limits to the artifacts represented. We stated: "Since the fill is shallow compared to the time range represented, there was undoubtedly a considerable amount of mixing, plus the ever present chance of reuse" (Irwin and Irwin 1959: 15).

The dates 1440 B.C. (M–1004) and 1190 B.C. (M–1006) show good correspondence with a date from Signal Butte of about 2000 B.C. for a slightly earlier but similar complex. The date A.D. 810 (M–1008) comes from 70 to 74 inches below baseline from a hearth that was apparently dug in from above. The date fits Complex B and well illustrates the overlap problem. M–1005 (58–60 inches below baseline) comes apparently from the contact zone between Complex B and C arbitrarily established as 53 inches (Irwin and Irwin 1959: 12), which might be revised downward a few inches in view of the date.

Complex D was the only manifestation found in the lowest levels except for a single parallel-flaked point from gravels underlying the site. This complex was identified as some manifestation of the so-called Desert Culture with roots in the Great Basin. The date 2800 B.C. (M-1009) comes from an early level of this complex.

Besides chronological establishment of a sequence for a period little known on the Plains, the dates discussed above are important in terms of the methodology employed in the establishment of the complexes. This method derives largely from the scheme outlined by Willey and Phillips (1958) with certain modifications. It is based on the recognition that the culture-history of any region can be viewed in terms of complexes of artifacts. These units can be identified at single component sites or sites where the components are clearly isolated. Interrelations of the complexes within the region can then be established at stratified sites such as LoDaisKa. The radiocarbon dates received agree well with our estimates of age based on typological consideration in terms of such complexes and are thus a partial validation of the method.

Remains of maize of several varieties come from the LoDaisKa site. The earliest maize is of a morphologically primitive type called Chapalote by P. C. Manglesdorf and his associates. Maize cobs of Chapalote type and what is probably maize pollen occur earliest at LoDaisKa from 78 to 82 inches below baseline. Given the dates above, this level falls between 1140 and 2800 B.C., roughly 4000 years ago. This maize is morphologically very similar to maize of about the same age from Bat Cave in New Mexico. Earlier maize at Bat Cave is of a different type. How important maize was in the development of culture in the area is unknown, but maize growing necessitates certain sociological conditions, such as the return at least twice a year to the same spot. Whether this early maize at LoDaisKa was of importance in the introduction of maize to Eastern United States is unknown. Nevertheless, Bat Cave lies farther from the LoDaisKa site than does the eastern part of Iowa.

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HARVARD UNIVERSITY
Cambridge, Mass.
April, 1961

COMMENTS ON GRIFFIN'S REVIEW OF *THE EASTERN DISPERSAL OF ADENA*

WILLIAM A. RITCHIE AND DON W. DRAGOO

ABSTRACT

Ritchie and Dragoo criticize Griffin, who rejected in his review their hypothesis that Adena groups migrated to the East Coast and Northeast, for basing his judgment on a few poorly documented sites, rather than on the large and recently excavated Adena sites in the Upper Ohio Valley and the Chesapeake Bay area. They summarize the evidence from these sites in terms of the questions raised in the review.

GRIFFIN'S (1961) review of our monograph (Ritchie and Dragoo 1960) in the April issue of AMERICAN ANTIQUITY seems to us wide of the mark in both accuracy and manner of presentation (Sarton 1960). It fails to provide the reader with a straightforward and objective account of our work. We hope to remedy this in the following comments.

In the first place, as we stated in our preface (p. 5), we offered an hypothesis for the migration of some, not *all* Adena groups, to the East Coast and Northeast. Although Griffin finds this idea incredible, a number of our colleagues do not. After the appearance of our article of the same title in AMERICAN ANTIQUITY (Ritchie and Dragoo 1959), the late A. L. Kroeber wrote the senior author as follows: "A very convincing account of Adena in July A. A. Congratulations."

It is not, however, honest differences of opinion which prompt this commentary, but a whole host of inaccuracies and innuendoes of which only the most conspicuous will be remarked.

The stone mounds, to which Griffin objects, mentioned in our report were of minor importance to the problem. We stated that they could only cautiously be identified as Adena since their contents were too meager to make positive identification possible. We suggested that they were perhaps Adena on the basis that their traits were Adena-like, that Adena materials were found on the surface near them, and that they were in the center of Adena distribution in the Monongahela Valley. The identification of the Linn Mound as probably of Adena origin was made by Dragoo on the basis of the artifacts and the field notes of the excavation now in the collections of Carnegie Museum. In the report concerning the Linn Mound (Dragoo 1955), the difficulties of clearly identifying the cultural affinities of stone mounds as either Adena or Hopewell were acknowledged. Although most of the stone mounds in the upper Ohio Valley seem to be Hopewellian, some, like the Linn Mound, in the area of Adena distribution, have traits more commonly seen in Adena.

Nowhere in the report was the Monongahela Valley considered a significant center of Adena occupation as stated by Griffin in his review. The main Adena center in the upper Ohio Valley was around Moundsville, West Virginia. The Adena mounds in the Monongahela Valley were on the eastern margin of this center and represent the most eastern extension of Adena mound construction.

Griffin's point concerning whether or not Cyrus Thomas actually surveyed and excavated the Crall and other mounds in the Monongahela Valley does not concern us here. Thomas (1894: 494–6) reported the work and it has been accepted generally as a reasonably accurate report of the results for the time in which it was done.

Griffin's comments on the McKees Rocks mound reveal an apparent lack of familiarity with several studies

of the mound by men who have seen the materials and records in the collections of Carnegie Museum (Mayer-Oakes 1955: 145–53; McMichael 1956; Ritchie and Dragoo 1960: 20–1). It has been generally agreed that the primary and secondary layers of the McKees Rocks mound can be assigned to Adena while the tertiary level probably was Hopewellian. Griffin did not mention these recent extensive studies of the McKees Rocks mound although he used this mound to discredit the article under review.

The reviewer seems to base his comments upon an unpublished study on file at Carnegie Museum by Charles Snow of the very fragmentary skeletal remains from this mound. In McMichael's exacting study of this material he found reference by Snow to only one burial which was definitely identified as to cultural affinity, Number 28, Adena. The central burial, Number 26, was probably female, but the cultural provenience of this individual was not mentioned by Snow. The objects with this burial were all Adena types and not Hopewellian as stated by Griffin. The two relatively complete skulls out of five burials in the secondary mound, both showed pronounced occipital deformation like that seen on Adena skulls. Twenty-three of 31 numbered burials were found in the tertiary mound and were probably of Hopewellian origin, but these remains were so poorly preserved that this would be difficult to prove. Snow's comment that "several examples showed occipital deformation, and in one case to a pronounced degree" indicates that Adena peoples may also have been present in the tertiary level.

Griffin gives the impression in his review that except for the Crall Mound (in spite of his questioning of Thomas' part in the excavation) the only other mound securely Adena in the upper Ohio Valley was the Peters Creek Mound. In this instance he ignores the Natrium Mound (Solecki 1953), the Beech Bottom Mound (Bache and Satterthwaite 1930), the Grave Creek Mound (Narona 1957), the Welcome Mound (Setzler 1960), and the Cresap Mound (Dragoo 1959). It was upon the evidence from these large Adena mounds that our picture of Adena in the upper Ohio Valley was drawn and not upon the scanty evidence from the stone mounds as intimated by the reviewer.

In answer to the several objections raised in the third paragraph of the review in question, we would like to make the following comments: If the villages of Adena in the Ohio Valley are so scanty in materials, why should we expect extensive villages of only small groups of Adena in the Chesapeake Bay area? The Chesapeake Bay burials, associated burial complex, and artifacts have little or no resemblance to those of cultural groups that were present in the Chesapeake Bay area before, at the time of, or after the appearance of the Adena material.

Why should a local culture use almost entirely imported objects as grave goods? Were not the imported raw materials in Hopewell used for the manufacture of Hopewellian objects rather than imported as finished objects diagnostic of another culture?

We did not say that the Chesapeake Bay Adena peoples were "dispossessed western Pennsylvanians" as stated by Griffin. We merely indicated that the materials found in the eastern sites are more closely allied to the sites in the upper Ohio Valley, which also includes eastern Ohio and West Virginia, as well as western Pennsylvania.

We do feel that the movement of certain Adena groups occurred during a relatively short period of time, as attested by the close clustering of radiocarbon dates from the Maryland sites and the homogeneity of the materials not only in the Maryland sites but also at a recently discovered site in Delaware that was excavated during 1960 by the Delaware State Museum. As yet, there is no evidence for a developmental sequence of Adena culture in the East. If, as we have suggested, certain small groups of Adena people were responsible for the burials and associated objects in the Chesapeake Bay area during a specific time, most of the objects carried with them would have soon ended up as burial furniture and there would have been few items left for distribution to peoples with whom they may have come into contact. There seems to be no conclusive evidence for the presence of Adena objects or influence on contemporary or later cultures in the Chesapeake Bay area.

Griffin is particularly critical in considering "Adena in the Northeast," and he fails to mention a good part of the evidence we have adduced in support of our hypothesis that shatter groups went northward from the Chesapeake Bay region. For example, the text discusses (pp. 26-9, 59–62) and the map (Fig. 1) shows the discontinuous character of the distribution of burial sites, tubular pipe-bearing habitation sites, and surface finds of the diagnostic blocked-end stone tube. There is no evidence of "a population explosion," but much to suggest the rather rapid dissemination of small groups along the main waterways, probably by boat.

Admittedly, the data from certain districts are scanty. Nearly all the eastern Adena-Middlesex sites have been found by accident and none was scientifically investigated. Surface evidence was lacking, skeletal material is almost non-existent, and we have been obliged to work only with cultural fragments. All this, and more, was clearly set forth in the monograph. Although Griffin finds himself unable to recognize exotic and Adena type materials on such sites as Rosencranz and Long Sault, in particular, we can only suggest that the reader look at the data for himself. When 66 to 75%, or more, of a site's total known content is foreign to the area in which the site occurs, one can postulate, we think, beyond the mechanisms of ordinary trade and diffusion for a logical explanation.

The radiocarbon dates for the Maryland sites appeared to be reasonably accurate and the dating of these sites about A.D. 1 probably is correct. Griffin suggests, as did we in the report, that this is also about the time of Hopewell dominance in southern Ohio and the time of late Adena south of the Ohio River. Typologically the items in the Maryland sites are late Adena with probably Hopewell influence as attested by the stratigraphic sequence recently recovered from the Cresap Mound in the upper Ohio Valley. Adena did not exist long after A.D. 1 and

the dates for the Florence and Drake mounds of A.D. 529 and 782 respectively do not accord with the evidence as we now see it.

Many traits on the Adena list are useless as "diagnostic" traits for Adena or any other culture. Only a core of specific traits in a specific context can be used. The typological evidence indicates the eastern Adena materials are basically late Adena rather than representative of Adena's various periods. If materials from all stages of Adena were found in the East, then trade, diffusion, and so forth would be more plausible explanations than our suggested migration of certain groups at a specific time.

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NEW YORK STATE MUSEUM
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CARNEGIE MUSEUM
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ARCHIVES OF ARCHAEOLOGY

Occasional publication on Microcard of primary documentation of archaeological investigations and related materials pertaining to the New World. The series is published jointly by the Society for American Archaeology and the University of Wisconsin Press. Order from the University of Wisconsin Press, 430 Sterling Court, Madison 6, Wisconsin. Remittance should accompany order.

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REVIEWS

Edited by Donald Collier

Prehistoric Investigations in Iraqi Kurdistan. Robert J. Braidwood and Bruce Howe. The Oriental Institute of the University of Chicago, Studies in Ancient Oriental Civilization, No. 31, University of Chicago Press, Chicago, 1960. xxviii+184 pp., 8 figs., 29 pls. $5.00.

The lone-wolf archaeologist is no more. Environmental studies are as much the life and breath of archaeology as water is to a fish. And it is no longer satisfactory to bring in a few bags of dirt or shells or mammal bones to the natural scientist for examination. The natural scientist, be he a botanist, zoologist, geologist, or whatever, just cannot give an accurate appraisal of material unless he knows the conditions of collecting, the locale and various on-the-spot information, which the archaeologist, half super-man though he may be, simply cannot provide. This is the prime lesson to be derived from the publication under review.

This number of the Oriental Institute of the University of Chicago series will be one of the most useful sources for information to students of anthropology and its interrelated disciplines for some time to come — or at least until a more detailed report is issued. It is one of those publications whose prices seem little compared to their worth.

The publication presents the story of the first three field campaigns of the Iraq-Jarmo Project, led by Robert J. Braidwood of the Oriental Institute in the Kurdish hill country of Iraq. The expedition set before itself the general problem: "How are we to understand those great changes in mankind's way of life which attended the first appearance of the settled village-farming community?" (p. 1). Even in the present time when problem-oriented archaeological expeditions are the accepted rule — no one seriously goes out to just dig anymore — the problem the Iraq-Jarmo Project set before itself represents one which few investigators would have the courage to tackle, much less to pursue to completion. To say completion may not be the proper word, since Braidwood feels that a full understanding of the transition from the level of food-collecting peoples to the level of settled village-farming may never come to pass. However, we are tremendously indebted to this team of culture historians and natural scientists for all the insights they have given us into this most important issue. This publication, in advance of the completion of all the varied studies involved in the Iraq-Jarmo Project's three field campaigns, presents the tentative conclusions of the investigators concerned. Those of us whose appetites have been whetted by the papers already published by the personnel of the project will be pleased to have bound in this single volume the fruits of their efforts. The material presented includes the latest statements of the investigators at the time of writing, including data not found in their earlier writings, and brand new, original offerings.

The archaeological section includes short descriptions of more than a score of sites examined by the expedition over the seasons, including the most comprehensive report (12 pages) yet published on Jarmo. The name of one site, "Telegraph Pole 26/22" is a striking oddity among the lot of jaw-breaking Kurdish names — and suppose someone cut the pole down? The natural scientist contributions include: "Specialized Ceramic Studies and Radioactive-Carbon Techniques" by Frederick R. Matson, "Climate and Prehistoric Man in the Eastern Mediterranean" by Herbert E. Wright, Jr., "The Paleoethnobotany of the Near East and Europe" by Hans Helbaek, "A Review of the Archeological Evidence on Animal Domestication in the Prehistoric Near East" by Charles A. Reed, and "Toward the Reconstruction of the Environmental Sequence of Northeastern Iraq" by Reed and Braidwood. The remainder of the work deals with straight archaeological problems and cultural-historical interpretations by Braidwood and Howe. We cannot deny the statement (p. 175) that "The Iraq-Jarmo Project has certainly made a far greater effort in a cross-disciplinary sense than has ever been attempted in the field in southwestern Asia." The step taken to bring the natural scientist into the field so that he can collect the related information for himself is an ideal, which may never reach the realization by all of us — and it is here that our hat goes off to Braidwood for his ability to focus together in a foreign land, at one time, at one spot, on one problem, some of the world's best authorities in their separate fields. The follow-up of the several concurrent investigations requires as much skill as getting the specialists assembled on one area problem in the field.

One of the major questions in the book concerns the status of the conception of the now famous Braidwoodian "hilly-flanks zone" as a nuclear zone for the domestication of plants and animals. The authors, one gathers, were not impressed with the evidence for an oasis theory of the origins of cultivation, smacking against Childe's point of view. This nuclear core area was extended by the University of Chicago expedition to Persian Kurdistan in 1959–60, where Braidwood and his colleagues found evidence quite comparable with, and even better than, their data from Iraqi Kurdistan. Braidwood now extends the region of nuclearity for the first appearance of plant and animal domestication and the establishment of a settled agricultural community to include an area from near Shiraz in Western Persia to at least northern Syria (*Illustrated London News*, No. 6325, Vol. 237, Oct. 22, 1960). This habitat zone is marked by an environmental area of grassy and open oak-land hill flanks of the Zagros from perhaps 2000 feet to 4500 feet elevation. Therefore the previous sites, such as Jarmo and Karim Shahir, are not in any way unique instances; they are part of a contemporary growing body of cultures moving in the same direction.

We are all familiar with the theoretical framework of developmental stages sequence upon which the authors have stretched their data, based upon subsistence and

settlement phenomena. This is the result of several trial formulations which we recall have undergone changes and shifts from time to time, but with the emphasis remaining the same.

I am for one a little happy to see that Braidwood has relented a wee bit regarding "Grecisms" and allowed the terms Middle Palaeolithic and Upper Palaeolithic to take their place in this publication. The old terms have become fossilized in the archaeological literature, and even though there is inaccuracy, these terms are really not such bogey men after all. The other terms are all familiar to us by now: food-collecting, incipient cultivation, primary village-farming community. One could quibble about their definitions as well, but since they serve as fair enough handles, the business of semantics need not befog the archaeology.

One point I would like to raise, since my own work is concerned here, is that contrary to the observation that there was no great change in climate as manifest in faunal and other data, we can now say on the basis of Mme. Arlette Leroi-Gourhan's fossil pollen data and Richard Shutler's corroborative trace element data, that there were marked fluctuations in the climate in Shanidar valley as evidenced by the Shanidar Cave sequence — not the least of which is the shift in climate from a cold environment to the present hot one following the last recession of the glaciers. This confirms Wright's geological observations. But, apologetically, these are new data, and are something to be added to the record.

In the complaint column, I might say that the organization of the volume is somewhat unusual; bibliographic references are given at the front and not in the rear. Also, more unfortunately, there is no index in the volume, restricting its usefulness. Further, one would wish for some section diagrams of the sites concerned as an aid to study and correlation. It is suspected that these could not be presented in the present work, which does have the appearance of an enlarged interim report.

For the last word, this publication is an extremely valuable addition to our knowledge of culture-historical and natural-scientific studies in the Near East.

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Ethnographic Bibliography of North America, Third edition. GEORGE PETER MURDOCK. Behavior Science Bibliographies, Human Relations Area Files, New Haven, 1960. xxiii+393 pp., 16 maps. $6.75.

This book is an exceedingly useful tool for the anthropologist working with the data of North America, whether he be ethnologist or archaeologist. I have used the first edition of this work for some 18 years. The first edition, published in 1941 contained about 9400 entries. The second edition, published in 1953, contained about 12,700 entries, but was somewhat difficult for me to use because of smaller type. The new third edition has more than 17,300 entries and has a large type face.

Like the two previous editions this new ethnographic bibliography is divided into 15 culture areas each of which is subdivided by tribal groups arranged in alphabetical order. At the beginning of each of the 15 major divisions there is an easily readable map showing the tribal distribution for that particular culture area. This is a useful innovation that was lacking in previous editions. There is also an index of tribes and a general map of tribal locations in North America north of Mexico.

This book should be in the hands of every archaeologist interested in North America not only because of its utility to archaeologists as such, but also because the archaeologists are among the relatively few anthropologists left who are interested in ethnology and tribal cultures. This of course is a healthy condition for archaeology, and I predict that ultimately archaeologists will be the practitioners of what I knew as ethnology in my student days 25 years ago.

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Gold in Alt-Amerika. Eine Einführung in die "Goldkammer" des Hamburgischen Museums für Völkerkunde und Vorgeschichte. WOLFGANG HABERLAND. Wegweiser zur Völkerkunde, Heft 4, Hamburgische Museum für Völkerkunde und Vorgeschichte (Edited by Franz Termer), Hamburg, 1960. 32 pp.

This paper-back without illustrations is an ambitious and generally successful attempt to cover pre-Columbian metallurgy and metalworking techniques in 23 pages. To achieve this the author has had to assume that his audience has something more than a nodding acquaintance with metal technology.

If I read it correctly, there is one major technical slip. On page 14 reference is made to "gilding" non-metallic objects by an impossible process that "corresponds exactly to cire perdue casting, but with the difference that here the wax is expelled by the molten metal." The "difference" suggested is unthinkable. To pour molten metal into a mold (and a mold or outer shell would be necessary) containing wax would be courting certain disaster. The intense heat of the metal would cause the wax to volatilize and expand instantly with explosive force, spattering molten metal all over the place. Cellini and other metals men have all stressed this danger and the necessity of *first* burning out every trace of wax or moisture *before* pouring.

Other technical lapses arise mainly out of needless speculation. It may be helpful to cite a few with comments. The pre-Columbian workers were more competent in smelting ores than Haberland suggests (p. 6; compare Caley and Easby, 1959, *American Antiquity*, Vol. 25, No. 1, pp. 59–65). After examining the Chibcha *tunjos* in the Museo del Oro in Bogotá, Pérez de Barradas and I are satisfied that all are cire perdue castings. The rough surfaces do not rule out cire perdue (p. 10); they merely

indicate that the wax model was not "faced." Herbert Maryon (1949, *American Journal of Archaeology*, Vol. 53, pp. 104 ff.) has explained the practical impossibility of fusion welding in making ancient jewelry, especially filigree work (p. 11). No reason exists for suggesting that soldering with copper salts and resin might have been the only method employed in the New World (p. 12); Sahagún mentions a hard solder made by adding a little copper to silver. While not inconsistent with the use of copper salts, the Tomb 7 and other Mixtec pieces I have examined show visible evidence of the use of a solder different in color and composition from the parts joined. There is no evidence that the Ecuadorean method of making bi-metallic pieces described by Bergsøe was used elsewhere (p. 13); on the contrary, X-ray and microscopic examinations of the three known Mexican gold-silver pieces disclosed that in one the lower melting-point silver was "cast on" to the finished gold half in a separate casting operation, while in the other two from Tomb 7, the two metals were joined by hammering. Microscopic examination of Mixtec collections in the Regional Museum of Oaxaca, the National Musem of Mexico, and the American Museum of Natural History did not reveal a single instance of "true filigree" (p. 24) — such work was all "cast wire" or "false filigree."

Omissions are inevitable in a condensed general survey, but the failure to note the unrivalled mastery of the Mixtecs in delicate "false filigree" and in minute hollow castings is astonishing.

These are relatively minor matters which can be corrected in a later edition. However, they do call to mind some sound advice Herbert Maryon once gave me about the danger of trying to make a diagnosis in the absence of the patient.

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Etnología y Arqueología. Publicación del Instituto de Etnología y Arqueología, Universidad Nacional Mayor de San Marcos, Año I, No. 1, 1960. Lima.

With this issue the Institute of Ethnology and Archaeology of the University of San Marcos introduces a new publication series, which for the present is to appear once a year. The purpose is to present the results of a new orientation in scientific investigations in the fields of the ethnology and archaeology of Peru that was introduced to San Marcos University with the origin of the Institute in 1946. Contributors to the series include students and faculty of the Institute, and those foreign investigators who have collaborated with the Institute or who have contributed something to the research in which the Institute is interested. The first number contains four major articles on ethnological topics and three that deal with archaeological ones.

The archaeological contributions are distinguished by the high quality of their scientific standards. They are: "Huaral: Inventario de una tumba saqueada" (56 pp., 17 figs., 6 pls., 3 tables) by Rosa Fung Pineda, "La cultura de Wari" (98 pp., 16 pls., 4 tables) by Luis Guillermo Lumbreras, and "Una colección arqueológica de Arequipa en la Universidad de California" (9 pp.) by John H. Rowe. All these articles were finished and submitted in 1958, though the date of the issue is 1960. As a result, Lumbreras' article has been superseded by a later one by him published in Vol. 28 (1959) of the Revista del Museo Nacional of Lima, and further information bearing on Rowe's article has come to light as a result of work done under the Fulbright Program and by students of the University of Arequipa during 1958 and 1959.

Rowe's article is an attempt to sort out styles from the far south of Peru supplementary to the ones classified by Kroeber in 1944. From the pottery classified by Kroeber as "Churajón" Rowe singles out several deviants, including two which he groups as separate and unrelated types. Work by Gary S. Vescelius and Máximo Neira A. conducted since 1958 in the Arequipa region confirms this separation. One of the types separated by Rowe (Kroeber 1944, Fig. 2 *A*, *B*) belongs to a style found on Inca-period sites in the area of Chuquibamba and the Sihuas valley, and the other (Kroeber 1944, Fig. 3 *B*) belongs to a Late and apparently pre-Inca pottery style found at Pocoma on the coast north of the Ilo valley, with stylistic resemblances to the Arica pottery of northern Chile.

The article by Rosa Fung is a description of the contents and archaeological associations of a looted but isolated Late pre-Inca tomb near Huaral on the north side of the Chancay valley. While the main emphasis is on the description and technological analysis of the textiles, attention is also devoted to the pottery and other artifacts. The article is distinguished by its clarity and precision, the excellence of the illustrations, the thorough background information on textile techniques that the author uses in her study, and, above all, the use of precise data on archaeological associations, a type of information which is rarely available in studies of Peruvian textiles.

The article by Lumbreras is the most ambitious work in the issue. It is primarily a reclassification of the pottery from the important site of Huari in the central highlands based on a part of the large unstudied collections made by Julio C. Tello's expedition to Huari and sites in the Ayacucho region in 1942. For evidence of associations to support his typology and seriation, Lumbreras uses data from his own field work carried out in 1957, both published and unpublished information on Tello's work, on Wendell C. Bennett's excavations of 1951, and on research done on the associations and seriation of the Nasca and Ayacucho styles since 1952 by members of the University of California and the Columbia University expedition to Nasca. The article is organized in three parts. The first part contains sections on the location of Huari and earlier reports and theories about the site, earlier archaeological work done there, descriptions of the site, the architecture, stone carvings, burials and other nonceramic remains. The second part deals with the pottery of Huari, and the third part is an annotated bibliography of published and unpublished reports about the site and the archaeological remains.

The pottery study is the most important feature of the article. It has to be read with close attention, because the classificatory divisions made in the text and on the charts do not coincide exactly (compare "Conchopata" and "Transicional"). Lumbreras starts out by making a typological segregation of Huari pottery, which is not as rigorous as might be desired. It is an improvement on Bennett's typology primarily because Lumbreras makes much better use of differences in site associations. The types that Lumbreras isolates are essentially Bennett's types, except for the distinction of two separate "Huarpa" types and a "Transitional" Late Nascoid type (in his later article included in a Late "Ayacucho" type). Lumbreras makes some important corrections of Bennett's classification in grouping individual pieces, but he adds errors of his own, both in classifying individual pieces and in general method. As an example of the latter, he puts Bennett's "Acuchimay" and "Ayacucho" types into a single stylistic category ("Ayacucho") because he has not found them segregated in surface collections. By this procedure Lumbreras lumps some entirely distinct types in a single style simply on the grounds that he believes them to be contemporaneous, a confusion of time and style which he professes to avoid in his introductory sections.

In grouping the different pottery types in units of contemporaneity and seriating these units in a chronological sequence, Lumbreras uses differences in site associations in the Ayacucho and Huari regions, data on associations and the seriation of Late Nazca pottery on the south coast, and data on the seriation of the Tiahuanacoid "Conchopata" type with reference to other Tiahuanacoid types. As a result of these data he proposes a chronological sequence which reverses the one proposed by Bennett, by putting the "Huarpa" style as the earliest one at Huari, the Nascoid "Ayacucho" and "Okros" types and the Tiahuanacoid "Conchopata" type as next, and the Tiahuanacoid "Wari" types as last. In further support of this sequence, he makes an observation which he credits to Edward P. Lanning, to the effect that the one pit in which Bennett found the "Huarpa" style isolated stratigraphically below the rest of the pottery at Huari represents the original superposition of refuse, and is not a case of reverse stratigraphy as suggested by Bennett.

There can be little doubt that the chronological sequence which Lumbreras proposes is basically the correct one. It suffers only insofar as his typology is inconsistent or fails to reflect the correct historical associations. Lumbreras also makes some other valuable points. He places the Cajamarca style in the latest period at Huari because (though he does not explain this clearly) Cajamarca style pottery is found at Huari, but not at the earlier sites in the Ayacucho region which also lack the "Wari" types. This observation has important chronological implications for cross dating with the Cajamarca sequence. The illustrations, made by Félix Caycho Q., are excellent, and the presentation of much heretofore unpublished background information on Tello's expedition is extremely helpful. The annotated bibloigraphy, which is exhaustive and which includes many references not generally available outside of Peru, is most informative and valuable. All told, this is an important contribution to the archaeology of the Ayacucho-Huari region.

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Nahrung und Nahrungsgewinnung im vorspanischen Peru. HANS HORKHEIMER. Bibliotheca Ibero-Americana, Band II, Ibero-Amerikanische Bibliothek (edited by Hans-Joachim Bock), Colloquium Verlag, Berlin, 1960. iii+157 pp., 7 figs., 8 pls., map.

The title, "Food and food production in pre-Spanish Peru," well describes the scope of this book. It is a compilation of data from the literature, supplemented by archaeological observations from the author's own experience on the coast of Peru. The German edition is a much expanded version of an earlier mimeographed one in Spanish entitled *La alimentación en el Perú prehispánico y su interdependencia con la agricultura*, published by UNESCO's Programa de Estudios de la Zona Arida Peruana in Lima in 1958.

Horkheimer has done a careful and conscientious job of surveying the literature on ancient Peruvian subsistence, and his study is a valuable guide to it. His bibliography takes up nine pages and includes many items of great interest which have not previously been used by archaeologists; it is, nevertheless, not exhaustive. The reader should bear in mind that, quite understandably in a survey, the author frequently reproduces the conclusions of earlier writers without going back to the original evidence. The judgment of some of these earlier writers is much less sound than Horkheimer's.

Horkheimer did not use G. E. Hutchinson's *The biogeochemistry of vertebrate excretion* (1948), and so failed to note that all historical records of the use of bird guano for fertilizer refer to the area betwen Moquegua and Iquique, with the exception of a highly dubious reference by Acosta to the use of guano in Cañete. The farmers of the south coast of Peru, from Chilca to Ica, at least, used fish for fertilizer. There is no positive evidence known to me for any use of fertilizer at all on the north coast. The Moche vessels which show visits to the guano islands reflect trips for sea mammal hunting and probably worship, while the archaeological specimens found in the guano deposits are most easily explainable as offerings.

The most valuable new archaeological information presented in this book is a plan and description of the ancient irrigation works at Hacienda Guadalupito on the right bank of the Santa River (pp. 118–21, Pls. 5, 6). The author also presents some information derived from an hacendado regarding a stone wall 5 km. long near Picamarán in the upper Cañete drainage that his informant thinks was a barrier for use in vicuña drives (p. 51).

Horkheimer presents a few ideas of his own which are of interest. Most important, perhaps, is the suggestion that in ancient times there was a distinct variety of llama which could be raised on the coast (pp. 42–3). This idea

rests on the observation that Moche representations of llamas show animals with a shorter neck than is found in modern sierra ones, and on the very large numbers of llama bones which are said to occur in coastal sites. Horkheimer suggests that the extinction of the coastal variety, if such a thing existed, was a consequence of a great epidemic in 1544–45 mentioned by Garcilaso (I, viii, 16). This theory that there was a coastal variety of llama deserves the most careful investigation.

Another suggestion of Horkheimer's is that the Inca Chimu blackware vessels that are so shaped that they represent a stubby *taklla* (foot plow) without a foot rest are, in fact, reasonably faithful models of short hand implements used in cultivation, rather than of the foot plow proper (p. 18, Pl. 3). This idea also deserves investigation.

In the section on nutrition there is a table of analyses giving the food values of 100-gram samples of a few native Peruvian foodstuffs. These analyses were provided by the Nutrition Department of the Ministerio de Salud Pública. The brevity of the list is a melancholy reflection on the lack of interest shown by Peruvian government bureaus in the native food resources of the Andes; no figure was available on llama meat, for example. In this case, as in many others, Horkheimer's survey serves to throw into sharp relief the many gaps in our knowledge of ancient Peruvian subsistence.

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O *Sambaqui do Macedo, A.52.B — Paraná, Brasil*. Wesley R. Hurt and Oldemar Blasi. Universidade do Paraná, Departmento de Antropologia, Arqueologia No. 2, Curitiba, 1960. 98 pp., 22 figs.

The University of Paraná has become an important center of archaeological research and teaching in Brazil with the establishment of a Museum of Archeology at Paranaguá, the sponsorship of field work, and the publication of a monograph series.

In 1958 Wesley R. Hurt, with the assistance of Oldemar Blasi of the University of Paraná, supervised archaeological work at a shell midden, known locally as Sambaquí do Macedo, near the small community of Alexandra in the Município de Paranaguá, Paraná. The excavation, which consisted of a trench 16 m. long, 8 m. wide, and 8 m. deep, produced 654 specimens (480 stone, 45 bone, 62 shell). In addition, 67 objects of baked clay, including sherds and tile, were recovered that relate to recent Brazilian (caboclo) occupancy of the site. The largest series of artifacts was axes, classified into subtypes of flaked, semi-polished, and completely polished. Other artifacts included hammerstones from waterworn cobbles and those with small depressions or pits on the surfaces, roughly flaked choppers, conoidal grinding stones, crude flaked and also ground knives, and elongated pointed pebbles probably used as picks. These stone artifacts are subdivided into complicated groupings and given an alphabetical and numerical designation for "quick" (?) reference in the charts. Bone objects included projectile points, perforated fish-vertebrae beads, and knives from ribs. Shell artifacts consisted only of beads and pendants.

The report is divided into short, concise, and well organized chapters: geographical description of the coast of Paraná; description of the shell mound and its local geographical conditions; the techniques of field work and laboratory analysis of the specimens; stratigraphy of the midden from a study of the physical condition of the layers; classification, tabulation and description of the artifacts; burials; comparison of this shell midden with others in Brazil; summary and conclusions; bibliography; and four appendices on detailed descriptions of the artifacts, the burials, the molluscs, the pollen and diatoms in the midden refuse. The report is adequately illustrated throughout with superbly clear and well-drawn ground plans, profiles of the strata, excellent line drawings of the artifacts, fair photographs, and detailed tables and charts of the occurrence of artifacts by levels. The presentation of the data and description of problems and techniques of excavating a shell mound should stand as an excellent model for future reports in Brazilian archaeology that deal with the highly complex and important problem of shell middens.

The inhabitants of Sambaquí do Macedo subsisted mainly on shellfish. The authors identify the various molluscs by strata and attempt to show changes in preference for certain species in various levels and part of the midden. The most outstanding shift is the decrease in the frequency of *Ostrea* (oysters) in the upper layers as compared to their abundance in the lowermost levels. This effort to show the frequency and presence of certain species and their relationships to different ecological conditions in the area has important implications in working out climatic shifts in Brazil and South America as a whole. The difficulty lies in the lack of such comparative data from other shell middens in Brazil, Uruguay, and Argentina. Fishing was also an important source of food, but no fishhooks were found. Animal and bird bones were very rare in the refuse.

Hurt and Blasi make a valiant effort to correlate the meager geological and ecological data available for the Brazilian coast with the archaeological information obtained from the excavation of Sambaquí do Macedo and other Brazilian shell middens. They have established a highly tentative, but reasonable, functional, and ecological scheme of cultural development of shell middens of the south Coast of Brazil, expressed in four phases:

Phase A. About 5000–10,000 years ago during the Climatic Optimum period when the ocean was lower. Lowest levels of shell middens that accumulated during this time are now a meter or more below the present sea level. Contrary argument against this extreme age is given by Bigarella who insists that the weight of the shell middens could push it into the swampy muck surrounding the sites and hence place the levels below sea level.

Phase B. About 3000–5000 years ago at the climax of the Climatic Optimum period when the sea re-invaded areas and developed ideal ecological situations for the

people living on shellfish. The large number of shell middens along the bays of Paranaguá and Guaratuba, as well as Sambaquís do Macedo and Araujo II, are assigned to this period.

Phase C. About 2000–3000 years ago at the period of a rapid lowering of the temperature and sea levels, known as the Little Pluvial period, and probably correlates with the sandy-clay levels in the middle of mangrove swamp deposits in various middens.

Phase D. About 2000 years ago to the present during a period of general warming correlated wtih the rise of the sea, as much as 2.5 m., in southern Brazil. To date no new shell middens can be attributed to this period but apparently Indian groups with pottery lived on the tops of already existing middens adding additional refuse.

The authors believe that the closest relationships of Sambaquí do Macedo, from the standpoint of the same species of shellfish and of the complex of artifacts, are with Sambaquí Araujo II on the coastal plain of Guaratuba, Paraná (Orssich and Orssich, 1956, *American Antiquity*, Vol. 21, No. 4, pp. 357–69). One problem not clearly solved at Sambaquí do Macedo is whether this and other Brazilian shell middens were occupied continuously throughout the year or only seasonally, and if seasonally, in what months they were used. Unfortunately, the accounts of the early chroniclers offer no aid. Hurt and Blasi tend to agree with Krone's estimate that large middens could be built up in a short time by people whose main source of food was shellfish. Radiocarbon tests should clarify this point.

This important monograph, and a few other studies in recent years, have pushed the study of shell middens in Brazil from an era of haphazard digging and armchair speculation about *"sambaquis"* in general, as if they were all related, all of a single culture and all of a single, early age, to an era of scientific archaeology. It is hoped that this outstanding work of Hurt and Blasi will encourage archaeologists to conduct future research on the shell middens of the Atlantic Coast of South America in order to solve some of the problems outlined in this report. The report can be obtained on an exchange basis from the Departamento de Antropologia, Universidade do Paraná, Rua General Carneiro, 460–6° andar, Curitiba, Paraná, Brasil.

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Prehistoric Ceramics and Settlement Patterns in Quintana Roo, Mexico. WILLIAM T. SANDERS. Carnegie Institution of Washington, Publication 606, pp. 155–264, Contributions to American Anthropology and History, No. 60, Washington, 1960. 110 pp., frontis., 19 figs.

In 1954, Sanders briefly visited some 40 sites in Northern Quintana Roo, returning in 1955 for a season of more intensive testing at Tancah, Tulum, Ichpaatun and Calderitas. A preliminary report, "An Archaeological Reconnaissance of Northern Quintana Roo" (*Carnegie Institution of Washington, Current Reports*, No. 24, 1955) and the present paper must be considered together in appraisal of the results. The emphasis of the survey was overwhelmingly on ceramics.

Analysis of the extensive collections from Tancah and Tulum was extended to smaller samples and surface collections to estimate the chronological position and geographical relationships of a number of more briefly visited sites from Chetumal Bay in the south to the Laguna de Yalahau in the north. As Sanders notes, these cross-datings serve to fix in time the samplings from the briefly visited sites, and should not necessarily be construed as delineating the life-span of the sites themselves.

Sanders' chronological framework for coastal ceramics agrees closely with the conclusions of my more limited architectural-ceramic excavation in 1956 at Xcaret (Andrews, 1957, *American Antiquity*, Vol. 22, No. 3, pp. 333–4) presently being prepared for detailed publication. At the base of Sanders' column, his Tancah complex, which he believes to run from late Formative times through the first phase of the Early Period (Tzakol), equates with a scattering of pottery at the base of the Xcaret stratigraphy which R. E. Smith (who was kind enough to make a preliminary analysis of our collection) identified as closely allied to Uaxactún Chicanel, and later with a number of polychrome sherds on the basal-flange horizon directly associated with major architecture. Lothrop was correct in his early assessment of Tancah architecture as earlier than that of neighboring Tulum. And although the suggested life of the Tancah complex continuing from late Formative into the first phase of the Early Period may seem unorthodox, recently discovered material at Dzibilchaltún seems to indicate the survival of a number of earlier wares into what was called the "Classic" in Yucatán, and possibly the direct evolution of the slatewares from these earlier forms.

Slatewares, a rarity at most of Sanders' sites, would seem to be the criterion of his second complex, named San Miguel. Its complete absence in the stratigraphic trenches at Tancah would argue a very early date for the major architecture at that site. However, the lack of habitation remains during the long period from his pre-Slate Tancah phase until the much later Decadent (Tulum) would seem strange. At Xcaret, not far to the north, over two-thirds of the pottery sample was Pure Florescent slate, clearly associated with major architectural activities. Slatewares in abundance were found by Sanders only at San Miguel on Cozumel Island, for which Xcaret was almost certainly the mainland point of embarkation. As Xcaret is the only point known to me where typically Pure Florescent carved stone mosaic is found in East Coast architecture, one wonders if these diffusions might not be features of trade along the pilgrimage route to the sacred island of Cozumel (which it surely was in later times) — in which case the San Miguel complex would be merely a limited intrusion of foreign material.

Equally significantly, Sanders points out the lack in Quintana Roo of the complex of ceramic and architectural traits characterizing the Modified phase of the Flo-

rescent ("Toltec") in Yucatán. This was equally true at Xcaret. And at other sites on the coast where Highland Mexican traits occur (such carved serpent columns, battered basal zones), they appear in the degenerate form inherited by the later architects on the Mayapán level of time, when we know there was an intimate cultural contact between the two areas. Sanders was mistaken in stating that the Modified Florescent tradition was "almost entirely limited to the site of Chichen Itza." It is widely spread over Yucatán, forming for example, an important horizon at Dzibilchaltún.

A type of unstriated bowl or jar which Sanders variously refers to as Alegre Vista ware, the Alegre Vista complex, and the Alegre Vista–San Miguel complex is described as having spanned this otherwise long hiatus of the Pure and Modified phases of the Florescent Period in Yucatán. Although this may well have been the case, one is inclined to suspect either that there are as yet undiscovered developmental horizons in East Coast culture, or that Early Period traditions survived much longer in this area, and that those characterizing the Decadent Period in Yucatán might have been of greater antiquity here.

The earliest component of Sanders' final Tulum complex, which he calls Mayapán Black-on-Cream, although it was indeed found at the base of the late stratigraphy at Mayapán, has been defined as a separate period at Dzibilchaltún, where it occurs in pure deposits associated with distinctive architecture clearly transitional between the mosaic-decorated, veneered concrete of the Modified Florescent, and the crude stucco-decorated, block-wall, and slab-vault construction of the Decadent (Mayapán) Period. Occurring at both Tulum and Tancah, this serves to narrow the gap at Tancah between the earlier occupation and that contemporaneous with Tulum.

The complete absence of anything pre-dating the Black-on-Cream pottery at Tulum in Sanders' very large collection establishes the Decadent Period date of the mass of well-preserved architecture at the site. I confirmed this by an examination of the broken wall surfaces of Tulum buildings, most of which show abundant fragments of Tulum redware re-used in primary construction mortar (although this was the case in only a few of the later structures at Xcaret).

Regrettably scant attention was paid to the important architectural remains at the many sites visited — much of it newly discovered. Although thorough recording of these finds would have been of greatest value, only rudimentary plans and simple elevations were made, and these only of some of the buildings examined. Little structural data are given, and no masonry sections. In publication, both drawings and photographs are reproduced in such minute scale as to be difficult and sometimes impossible to use (in the earlier report 45 line cuts including eight site maps were reduced to three 6-by-9 inch pages; in the present contribution, the 30 architectural photos are reduced to two figures). The future worker interested in architecture and art as well as ceramics will have a long and very difficult trail to retrace to fill in essential detail which would have consumed relatively little time on the survey — and already much of the material has been demolished.

Sanders is to be congratulated on the results he accomplished towards his set goal. He has published a clear account of the basic pottery wares in an area previously unknown to the ceramist, and assembled the resultant material into a convincing series of complexes which he was able through stratigraphy and seriation to set in chronological order. Finally, I am in accord with the pattern of relationship set up by Sanders to equate his material with that of neighboring archaeological areas. With a short time in the field, and a limited time to prepare his material for publication, Sanders has made a permanently valuable contribution to the Maya field. His information was profitably applied to series of gridded test pits to determine ancient settlement patterns at Tulum and Tancah, that is, what segments of the population lived at what parts of the sites, and at what times.

An interesting sidelight to Sanders' reconstruction of Quintana Roo prehistory is the increasing evidence of uninterrupted and relatively little influenced cultural development, particularly in architecture (much of which he has more firmly set in the time scale) during the profound cultural changes which took place in the western half of the peninsula during the two phases of the Florescent Period. In relative isolation in this area, art and architecture seem to have developed and then degenerated within narrow patterns into forms strikingly similar to those that appeared relatively suddenly after the disappearance of Florescent traditions in the west. I suggested in the Xcaret study (1957) that a survival of Early Period traditions on the East Coast might be responsible for the apparent return of these earlier techniques in architecture, art, and ceramics in the final stages in Yucatán.

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The Carved Human Femurs from Tomb 1, Chiapa de Corzo, Chiapas, Mexico. Pierre Agrinier. Papers of the New World Archaeological Foundation, No. 6, Publication 5, Orinda, 1960. vi+26 pp., 17 figs., $2.00.

The two elaborately carved bones found at Chiapa de Corzo, in Tomb 1, Mound 1, are of very considerable importance in Middle American art and history. They have been treated previously in two papers by Keith A. Dixon (1958, *American Antiquity*, Vol. 24, No. 1, pp. 53–62; 1959, *Archaeology*, Vol. 12, No. 2, pp. 106–10). Agrinier's publication offers additional description and discussion of these carvings. With the aid of detailed analytical drawings prepared by Ramiro Jiménez Pozo, whose name does not appear, he is able to present features not easily seen in his photographs of Dixon's rolled-out casts. Unfortunately, he causes confusion by changing the designations of the different bones. Dixon's Bone 2 has become Bone 3, and the plain ones with the carvings, Bones 2 and 4.

Agrinier identifies a "dragon," Figure A on Bone 1, with the head, Figure F, on Bone 3, and he differs with Dixon's conclusion that Figure F is not fleshless. Since the jaw treatment of the Figure F head is identical to that of "Dragon" A, Bone 1, and the latter is not interpreted as a fleshless death symbol, Agrinier's interpretation is difficult to accept.

Both Dixon and Agrinier assign Tomb 1 to Proto-classic, but Agrinier rejects the radiocarbon dating of the pottery complex and places it more than 200 years earlier. As additions to the discussion, Agrinier suggests for the figures Mexican and Maya hieroglyphic associations that rather stretch the imagination, and he provides a brief, naive discussion of aesthetics (pp. 21–2).

In examining the areal associations of the style of the portable Chiapa carvings, Agrinier follows Dixon's argument quite closely, although with greater emphasis on "Olmec" affiliation. Actually, the motifs of bearded "jaguar"-faced figures and "dragons" and the manner of their carving conform closely to a recently recognized Middle to Late Preclassic monumental tradition in the Guatemala Highlands and Pacific Slopes, the definition and full documentation of which is still unpublished.

There are some additional oversights of documentation in this publication. The fact that Agrinier's identification and discussion of the "dragon" and its Maya descendants was preceded by a discussion of the matter by Covarrubias (1957, *Indian Art of Mexico and Central America*, pp. 60, 81, Fig. 36) is not mentioned, although Covarrubias's book is referred to in other connections. The stela fragment from Kaminaljuyú, Guatemala, shown in Figure 8, is given a phase attribution without naming a source of information, although there is a published statement by Heath-Jones (1959, *Abstracts of Papers*, p. 37, 24th Annual Meeting of the Society for American Archaeology.

Detailed analytic studies of carving and sculpture are extremely important to the growing understanding of art and history in Middle America. Perhaps future studies of the Chiapa de Corzo carvings will be less repetitious and more comprehensive. It would have been useful to expand the comparative section by utilizing the data on human femurs from Uaxactún (A. V. Kidder, 1947, *Carnegie Institution of Washington, Publication* 576, pp. 57–9) and from Kaminaljuyú (Kidder, Jennings, and Shook, 1946, *Carnegie Institution of Washington, Publication* 561, pp. 153–5).

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Das Geschichtswerk des Domingo de Muñon Chimalpahin Quauhtlehuanitzin (Quellenkritische Studien zur frühindianischen Geschichte Mexikos). Günter Zimmermann. Beiträge zur mittelamerikanischen Völkerkunde, 5, Hamburgisches Museum für Völkerkunde und Vorgeschichte, Hamburg, 1960. 79 pp.

Zimmermann's translation from the Nahuatl into German of selected extensive parts of Chimalpahin's works had the two-fold purpose of determining what sources Chimalpahin had used to test the reliability of his chronicle and to present an account of the early history of the Mexica-Tenochca and the Chalca, particularly those parts which can be checked against other chronicles. These translations were not available when the author completed his manuscript (doctoral dissertation) in 1957.

In 1958 the greater part of Chimalpahin's writings, mainly those of significance to the Americanist, were published in German (*Das Memorial Breve . . .* translated and edited by Walter Lehmann and Gerdt Kutscher; reviewed by von Winning, *American Antiquity*, Vol. 25, No. 3, 1960, pp. 437–8).

Chimalpahin, who has been acknowledged as a foremost source for the ancient history of Mexico, wrote a century after the Spanish Conquest, but even at that time (between 1620–31) he still had at his disposal documentary evidence in Nahautl, as well as pictorial codices, many of which since have been lost. He is noted as the only chronicler of events in the southeastern part of the Valley of Mexico. A descendant of the former lords of Chalco, which was subjugated by the Aztecs in 1465, he had in his possession family documents which were used to attempt the restoration of the Chalco dynasty before and after the Conquest. For his concise evaluation of the sources employed by Chimalpahin, Zimmermann translated the 8th *Relación* (not included in the Lehmann-Kutscher edition), in which the chronicler commented at length on the material available to him, a feature not equalled by any other chronicler. This translation is of particular importance in judging the accuracy of the chronicle because it was written a long time after the Conquest. Proof of the reliability of Chimalpahin is derived from comparisons of passages taken from the geographical description of the world given by Enrico Martinez, who wrote in Spanish, and whom Chimalpahin translated accurately into Nahuatl. Versions of the 2nd and 3d *Relaciones*, furthermore, show that Chimalpahin followed Cristóbal de Castillo (who completed his *Fragmentos* in 1599), Tezozomoc (who wrote in 1609), and the Codex Aubin of 1576. Considerable passages were copied literally. For the history of the Mexica-Tenochca, during the period they remained in Chapultepec, Chimalpahin recounts events analogous to those in the *Anales de Tlaltelolco* (1528), although he did not copy them literally. Even though no comparisons with other sources can be made for the history of Chalco — except for certain toponymics and tribal names — Zimmermann, who translated extensive passages concerning these events (3d *Relacion* and *Memorial*), makes a clear case for the reliability of these reports. The most obvious weakness in the *Relaciones* lies in the dating of the early history of Culhuacán, because the migrating Chichimecs were in no position to record their history, which they reconstructed at a later time with the aid of records kept by peoples whom they subjugated.

Zimmermann's translation is more fluent and therefore more descriptive than the word-for-word translations by Lehmann and Kutscher. Certain linguistic pecularities of the Chalca-style Nahuatl are discussed and differences in semantics with the Nahuatl of the central and northern

Valley are pointed out. I am not qualified to undertake an evaluation of the discrepancies in the translation between this edition and the Lehmann-Kutscher edition which are admitted by Zimmermann in his epilogue.

Bibliographical analyses concerning sources written in Nahuatl have so far been conducted only by Radin (1920), whose findings have been largely superseded by later publications of numerous sources in native texts, and by Barlow (1945), who established the hypothetical "Cronica X" which must have served as a source for various later chroniclers. Therefore, this work and the author's plan to publish a complete translation of Chimalpahin's works in the light of a critical analysis of the sources are to be welcomed, particularly as they should clarify historical events and linguistic changes and should aid in the reconstruction of the "Cronica X."

Zimmermann's *Geschichtswerk*, although it parallels in some respects the Lehmann-Kutscher edition, is in no way superseded by the latter. It is, on the contrary, a valuable complement to the literal translation.

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Archaeological Investigations of Inland and Coastal Sites of the Katmai National Monument, Alaska. Wilbur A. Davis. Archives of Archaeology, No. 4, Society for American Archaeology and University of Wisconsin Press, Madison, 1960. xi+216 pp., 53 figs., 6 maps on 3 double-sided microcards. $1.25.

The Katmai project was organized by the National Park Service for the purpose of classifying the resources in the Katmai National Monument. Wilbur A. Davis and James W. Leach worked together under the direction of W. S. Laughlin to survey the Monument and excavate in certain specific sites. The report by Davis first appeared in mimeographed form in 1954. Between this date and its reissuance on microcard in 1960, the subject matter with which it was concerned was amplified by the description of a single Katmai Monument site excavated by Wendell Oswalt in 1954 (Prehistoric Sea Mammal Hunters at Kaflia, Alaska, *Anthropological Papers of the University of Alaska*, Vol. 4, No. 1, pp. 23–61, 1955). The present report and that of Oswalt together pioneered the archaeology of an area between the Aleutian Islands on one side and Cook Inlet and Kodiak Island on the other.

Twenty-three sites were identified, all of them holding one point in common. They were thought to have been abandoned in June of 1912 upon the eruption of Mount Katmai. The task of an archaeologist visiting the abandoned villages such as Kaguyak and Savonoski is unlike that of one examining other Alaska ghost towns, for here thick layers of volcanic ash have been deposited, perhaps hastening decay in such a manner as to give depressions the appearance of ancient sites. Equally in ruins at Kaguyak were the Russian church, two-room surface houses, earlier pit dwellings, and a ceremonial *kashim*. Test excavations produced a few artifacts of both European and native origin, but neither skeletons nor other material of a definitive nature.

The site of Kukak, on the other hand, proved more amenable to excavation. A long trench, bisecting an oval house pit in one place, exposed limited midden deposits similar to those of neighboring north Pacific sites. Kukak, and a site at Brooks River composed of four occupation mounds, yielded 446 artifacts. Present in both sites were harpoon dart heads, barbed at one edge; implements of ground and polished slate and feldspar, including fine long barbed polished arrow points and semilunar edged knives; insert polished blades for compound harpoon or arrow heads; abrading tools of sandstone or shale; planing adz blades; and pottery. Sixteen potsherds from Kukak, representing three vessels, were made of yellow or bluish clay tempered with sand gravel and vegetable fibers.

Four polyhedral cores bring up the question of whether some form of the blade and core technique persisted until modern times in this part of Alaska. Pebble sinkers with double notches in the long axis indicate the use of netting in an inshore fishing complex. The absence of the distinctive traits of surrounding areas made it difficult, however, for the author to define Katmai culture on a comparative basis. Lacking in the sites were distinctive forms of design or pottery treatment, toggle harpoon heads or other parts of the sealing harpoon, composite fish hooks, wood-working tools, or skeletal remains. In the absence of these kinds of traits that would crossdie with the sites of Cook Inlet or the Aleutians, it seems doubtful that the author's tentative comparison wtih Kobuk River sites far to the north justifies an estimate of A.D. 1500 for the earliest deposits in the Katmai area.

The thorough documentation in tables and appendices shows that the sites excavated were remarkably sterile in light of their position between the rich middens to the south and east of Katmai and the promising series discovered by Helge Larsen westward on the shores of Bristol Bay. One wonders why the Aglemute, who are indicated as the salmon fishers, walrus hunters, and sometime whalers who occupied these sites, were less productive of artifacts than their neighbors. The search for earlier sites was made difficult by the accumulation of volcanic debris. It seems just possible that the relative barrenness of the known middens and the paucity of earlier sites owes to some degree to the volcanic activity that must have long plagued the residents of Katmai.

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Archaeological Survey Annual Report, 1959–1960. Department of Anthropology and Sociology, University of California, Los Angeles, 1960. 330 pp., 11 figs., 69 pls., 10 maps, 30 tables.

This is the second annual report of the Archaeological Survey, a semi-autonomous branch of the Department of

Anthropology and Sociology, University of California, Los Angeles. The survey operated in 1959–60 under the continuing direction of C. W. Meighan with M. B. McKusick as Archaeologist and C. N. Warren as Assistant Archaeologist. Twenty projects were conducted during the year and four of these are reported. Some 820 man days were devoted to the work, 263 man days more than in 1958–59, first year of the survey. The area of concentration comprises the ten southern counties of the state, with particular attention to the Channel Islands and San Diego county.

There are eight papers, six by the staff and students, two by collaborators from other institutions. The longest paper concerns six sites on San Nicolas Island with brief notes on surface finds at other sites, a petroglyph site, shell work, and skeletal material from the sites. The second paper is an analysis of skeletal material from the Zuma Creek site on the southern California coast near Point Dume. The results of the study indicate the need for work on unreported series and restudy of material in collections in institutions in California and elsewhere.

The third paper is on a site at El Portal at the west end of Yosemite Valley, Mariposa county. This adds to data published in the papers of the University of California Archaeological Survey, Berkeley.

Of particular value is a brief report on a rockshelter in southeastern Ventura county, approximately 35 miles northwest of Los Angeles. In past years basketry fragments and a few whole baskets have been found in rockshelters in the hilly back country but few reports have been published. This paper by M. Kowta and J. C. Hurst is of use in establishing the context of earlier finds.

Two papers are on material from the western edge of the Imperial Valley–Desert Province of southern California. These papers are useful additions to others on the same general region, one of which was published in the 1958–59 annual report of the survey.

The seventh paper is by W. J. Wallace and reports a survey of the Buena Vista watershed, San Diego county. Such surveys of small geographic areas might well be encouraged so that some evaluation might be made of the archaeological resources and thus more adequate plans made for their investigation. The inclusion of an area map would have been helpful.

The eighth paper is by C. E. Rozaire and is on a site near Encino in northwestern San Fernando Valley. Possible relationships with the Tank site of Topango Canyon may indicate some antiquity of the Encino occupation.

This report is in the growing tradition of quick, although generally brief accounts which has developed in conjunction with archaeological salvage work in the United States. Separate reports on the more extensive projects of the survey are noted as in press. The general appearance of the report is good. Illustrations have been improved over those of the 1958–59 report. The quality of the maps varies. Some of the plates of artifacts lack a scale and there is some variety of style in scales used in plates of the separate papers. Each paper has a separate bibliography. In addition to the text each report has also tables, drawings, and photographs. Editorial work is adequate except for a few typographical errors.

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The Hohokam, Sinagua and the Hakataya. ALBERT H. SCHROEDER. Archives of Archaeology, No. 5, Society for American Archaeology and University of Wisconsin Press, Madison, 1960. v+209 pp., 10 figs., 10 pls. on 4 double-sided microcards. $1.65.

In these days when many Southwestern and other archaeologists are virtually immersed in salvage archaeology, a study more theoretically oriented to cogent problems is a most welcome addition. Such is Schroeder's important synthesis of the prehistory of central Arizona and the diachronic and spatial interrelations of the cultures in that semi-arid region from the Mogollon Rim west to the Colorado River and from the Gila Basin north to the San Francisco Mountains. In this study, published on microcard, the author brings to bear the results of practically all his research since 1938 and expands upon several of his previously published pertinent articles.

The first 81 pages of the report detail the archaeology of the middle Verde Valley surveyed by Schroeder from 1946 to 1950. These indicate a basic cultural pattern affected by the Hohokam from about A.D. 700 to 1150, a Sinagua movement into the valley from the Flagstaff area about 1125 to 1400, and a historic utilization principally by Yavapai and Apache.

After briefly discussing the history and environmental factors of the middle Verde, the author neatly documents his views with ceramic, architectural, and artifactual data from surveyed sites. It is perhaps unfortunate that he was unable to cite Breternitz's recent corroborative evidence (*Excavations at Three Sites in the Verde Valley, Arizona.* Museum of Northern Arizona, Bulletin 34, 1960).

A comparison of Hohokam and Sinagua traits in the text and in a comprehensive ten-page spatial-temporal chart, together with correlations with neighboring peoples, disclose significant conclusions. There is a high correspondence between Sinagua traits before 1070 and Pioneer period traits before 700 (although here Schroeder assumes cremation and portable mortar and pestle to have been present among Sinagua); the former should be considered as the northernmost extension of the Pioneer period culture far removed from Mexican influence. By 1120, shortly after the Sinagua had received Sedentary Hohokam traits from a colony settling just east of Flagstaff, Anasazi and Mogollon traits changed this basic pattern. As for the Pioneer period itself, Schroeder believes it should have an end date of 700, that it had no necessary connection with either Mogollon or the earlier Cochise culture; and that at 700 a new people, the Hohokam, entered the Gila Basin with new (Mexican) traits. A revised population estimate of 12,000 for the Classic

Period Hohokam of the Salt River Valley is also well supported, although more comparative material from other arid lands might have been considered. Finally, the Salado did not represent a single cultural pattern coming to Roosevelt Basin from the upper Little Colorado, but rather a mixture of some indigenous group and an influence from Hohokam to the south, Mogollon-derived peoples from the east, and possibly Sinagua.

Schroeder proposes a new root, the Hakataya, to account for the Pioneer period as well as several other indigenous groups in central Arizona. The Hohokam then would become a stem or branch exhibiting traits of the Colonial and Sedentary periods, while the Sinagua after 1120 would be referred to as a cultural pattern. The Patayan Root also should be accorded the status of a stem, including branches in the Agua Fria, Verde, Roosevelt, Prescott, Cohonina, and Sinagua regions, while the Laquish Stem is represented by branches such as the Amacava of the lower Colorado River. In short, Schroeder believes that Hakataya, a Yuman term meaning Colorado River, represented a group of traits "that appear to be basic to all of the [folk] cultures . . . on the lower Colorado River and in western and central Arizona in ceramic times only, as well as those in the California desert and the northern portions of Lower California" (p. 84), all of which at one time or another were affected by more complex cultures. The majority of these traits he believes survived into historic times.

To support this view Schroeder gives detailed trait lists not only for Hakataya, Hohokam, and Sinagua, but also for the historic desert tribes. He suggests prehistoric Yuman-speaking antecedents for many of these including Mohave, Yavapai, Walapai, and Havasupai (disagreeing in the second and last instance with Dobyns and Euler, 1960, A Brief History of the Northeastern Pai, *Plateau*, Vol. 32, No. 3; and Euler, 1958, Walapai Culture-History, MS, doctoral dissertation, University of New Mexico). The Gila River Pima he considers to have been Sobaipuri who overran the Gila-Salt about 1450, stating that between 700 and 1200 there were three patterns in southern Arizona: in Papagueria (Cochise derived), on the San Pedro (Hohokam and Mogollon derived), and on the Gila (Hohokam-Mexican derived). In so arguing Schroeder denies the existence of Haury's Desert Hohokam.

These hypotheses are on the whole well buttressed factually, with the possible exception of those linguistically oriented (for example, the Sinagua as Yuman speakers changing to Uto-Aztecan).

There really are many more data here than can be commented upon in a short review. Many archaeologists certainly will not agree with all of Schroeder's conclusions. But he has presented a report of marked significance and something of a challenge not only to archaeologists, but to all anthropologists concerned with the culture history of Arizona.

For this reason it is unfortunate that this study had to be published in the relatively new microcard format. I read the book on a Kodagraph Microprint Reader, Model A (currently one of the more expensive readers at $495), which projected each page of text to 5½ by 8 inches. The photographic reproduction left much to be desired; Figure 1 was illegible and all the photographs were obscure. To refer from text to chart or plate required removal from the reader of one card and substitution of another with subsequent loss of continuity. Some charts required more than one page. The editors of the *Archives of Archaeology* series should give consideration in the future to a layout placing illustrative material in greater proximity to the text to which it refers. These are, however, minor criticisms. Microcard publication certainly offers great advantages, especially for the presentation of raw data. But Schroeder's study was not raw data and, while the editors are to be commended for making it available, it is unfortunate that it was not published elsewhere in a more accessible medium.

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The Great Kivas of Chaco Canyon and Their Relationships. Gordon Vivian and Paul Reiter. Monographs of the School of American Research and the Museum of New Mexico, No. 22, Santa Fe, 1960. vi+112 pp., 38 figs., 2 tables. $2.50.

It is a historical fact that until recently monographs reporting the extensive field work that has been performed over the years in the Chaco Canyon area of New Mexico have been scattered and sketchy. As a consequence, it is particularly gratifying to be able to note that in the past six years this situation has been remedied considerably by the appearance of three well-written monographs: Neil M. Judd's reports on "The Material Culture of Pueblo Bonito" and the excavation of "Pueblo del Arroyo," both published in the *Smithsonian Miscellaneous Collections*, and Vivian's report on "The Hubbard Site and Other Tri-Wall Structures in New Mexico and Colorado" published in 1960 in the National Park Service *Archaeological Research Series*. To this growing list may now be added the present monograph.

The authors began this study about 1938 and had virtually completed it in late 1940 or 1941. Before submitting the manuscript for publication in its present form, Vivian brought it as completely up to date as possible, although Judd's report on Pueblo del Arroyo (1959) had not been released when this manuscript went to the publishers. Since publication, another great kiva of the Chaco Canyon type has been recorded near Sanders, Arizona, about 13 miles west of the great kiva at Allentown.

Three major topics are presented in the monograph. Part I is a report on field work performed in great kivas in Chaco Canyon. Part II is devoted to the definition and discussion of great kiva features, and Part III contains a discussion of the origin and history of great kivas within the framework of general Southwestern prehistory.

The field work upon which Part I is based was conducted over a period of more than three decades (beginning in 1921) and under the supervision of many different archaeologists. Associated pottery and artifacts for

some of the older excavations have been lost, or mislaid, and the same fate has befallen some of the photographs and field notes. In spite of these deterrents to accurate reporting, Vivian and Reiter have painstakingly put together fragmentary data, aided by re-photographing and by some re-excavation, and have reconstructed as complete a report as possible. Each of the eight great kivas is illustrated with plans, profiles, and photographs.

In order to provide a larger sample for the discussion of kiva features in Part II, the authors have included for consideration the 11 other great kivas that have been excavated or tested in the northern Anasazi area. About 15 great kiva features are discussed in considerable detail. The most persistent and distinctive of these features appear to be paired floor vaults, raised firebox, north antechamber, and four masonry roof supports, as well as more general characteristics such as size, a circular shape, a bench, and a determined effort to achieve subterraneity.

In Part III the authors present considerable detail on the great kivas excavated in other areas of the Southwest, in their discussion of the possible origins, development, and disappearance of the Chacoan great kiva form. The evidence presented suggests that this form was not derived from the Mogollon, nor was it a purely Chacoan development. On the other hand, it is suggested that the Chacoan or Anasazi version of the great kiva probably had broad Anasazi origins and later specialized development in the Chaco area.

The descriptive sections in this monograph are thorough and lucid. In the comparative sections and discussions the authors have given considerable thought to alternative explanations and hypotheses. These have been presented without undue bias, so that the reader is offered new data and interpretations of Chacoan archaeology in such a way that he may draw his own conclusions concerning them.

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Table Rock Pueblo, Arizona. PAUL S. MARTIN AND JOHN B. RINALDO. Fieldiana: Anthropology, Vol. 51, No. 2, Chicago Natural History Museum, Chicago, 1960. 171 pp., frontis., 91 figs., 2 tables. $5.50.

In 1956, after having worked for nearly two decades in the Reserve–Pine Lawn area of west-central New Mexico, Martin and Rinaldo moved their theater of operations to the vicinity of the headwaters of the Little Colorado River in east-central Arizona in order to elucidate some of the archaeological problems of this relatively unknown area. Table Rock Pueblo is a report on the 1958 season.

The Pueblo consisted of some 60 to 100 rooms arranged in two roughly parallel rows with a narrow, irregular "plaza" between. Fifty rooms and two kivas were excavated. Most of the rooms had been used for dwelling or storage but others may have been used for religious purposes. Both kivas excavated were of the rectangular, Western Pueblo type with a raised platform across the southern side. On a knoll a short distance from the pueblo several rooms and a pithouse that may have served as a kiva were also excavated.

With the exception of a negligible fraction, the pottery points to an occupation dating between A.D. 1300 and 1450. Plain brown, red, textured wares, and locally made Gila and Tonto polychromes constituted about 80% of all pottery found, while 7% consisted of Zuni Glazes and Pueblo IV Hopi types. The remaining pottery was largely undetermined as to type. Most of the native pottery consisted of varieties based on Alma Plain, but differing from the parent type in being slipped and sherd tempered. Even the local Gila and Tonto Polychromes were made on this Alma Plain base.

Stone and other non-ceramic artifacts generally follow the pattern known from other late Western Pueblo sites, but there were a few innovations, notably a slightly convex metate type. In general the findings point to a culture having a Mogollon base but showing increasing similarity to the late prehistoric Hopi and Zuni complexes.

This report is divided into four sections dealing with Pueblo Details, Pottery, Artifacts, and a Summary, which includes some conjecture. Descriptive matter is presented in expanded outline form, and for the most part, this presentation, coupled with numerous clear illustrations, allows for a maximum coverage of detail. In the chapter on pueblo details description is in terms of walls, floors, roofs and ceilings, and it is difficult to visualize any single room other than the kivas which are treated as units. However, these details are pulled together in the concluding part of the chapter and one emerges with a good idea of the total pueblo. On pages 173–5 there is a suggestive speculation on the number of kiva uses prompted by a group of worked sherds in a kiva niche.

Martin's discussion of the problems of pottery typology, particularly of the plain utility types, and his analysis of paste types to show the basis for establishing varietal status are especially clear. When comparable data are available from other sites and localities the processes of ordering of types and varieties in the Southwest will have made a long step forward.

For many years the reports of Martin, Rinaldo, and their associates have been clearly and concisely written, well illustrated, and promptly published. Yet the complaint has occasionally been made that the analyses and descriptions were somewhat mechanical and the results marred by a lack of meaningful interpretation, despite the fact that Martin and his colleagues spent a fair share of their pages in conjectural reconstruction. This complaint, whether or not justified in the past, cannot be made of this report. Most of the analyses here are highly competent, and the discussions, especially of the pottery, truly perceptive and meaningful. These qualities, when combined with a contribution to a relatively little-known area mark this report as a significant one for the Southwestern archaeologist.

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A Review of Texas Archeology, Part 1. Edited by Edward B. Jelks, E. Mott Davis, and Henry B. Sturgis. Bulletin of the Texas Archeological Society, Vol. 29 (1958), Austin, 1960. vii+254 pp., 9 figs., 22 pls., 2 maps. $5.00.

In 1954 the Texas Archeological Society issued the monograph, "An Introductory Handbook of Texas Archeology" (Bulletin, Vol. 25), which is now out of print. The present volume is a revision and reissue of a part of that handbook, and is intended to replace it.

The editors have set a logical, systematic style and format, and each of the five contributors has followed this style and format very well. The printing, paper, reproduction of plates, and format are excellent but the binding is not satisfactory. The cover won't stay fastened to my copy.

In the first section, William W. Newcomb, Jr., Director of the Texas Memorial Museum, presents a concise survey of the historic tribes of Texas. He attempts both a historic account and an ethnographic summary of each of the tribes and does it quite well. It is not only factual and informative but the writing style is clear and non-technical. The list of references directs the reader to a substantial source of technical material. This article is a most fitting introduction to the prehistoric backgrounds of these Indian groups and the areas they occupied. Each of the other authors includes a brief section on historic tribes as well, but this apparent duplication supplements rather than reiterates the article by Newcomb.

The second section is by Clarence H. Webb, of Shreveport, Louisiana, who has contributed richly to Caddoan area archaeology for many years. He offers a well-written synthesis of the archaeology of northeast Texas — the area, the sites, the organized studies, the complexes, suggestions for future studies, and exploration and excavation problems peculiar to the area. Webb's thorough command of Caddoan area prehistory as it has so far been developed is excellent. I am especially impressed by his lucid presentation of suggestions for future studies in the area. These are not generalizations but specifics. They are the basic problems of future research in this specific context and each is a real, but solvable, problem. I object to his use of the term Neo-American but this is a minor point.

Dee Ann Suhm, senior author of the "Handbook" and now with the Texas Memorial Museum, has summarized the Central Texas area very well. This is a difficult series of archaeological complexes to interpret. Suhm has demonstrated here not only a firm grasp of the current interpretations of this sequence but also presents a thoroughly fair and lucid review of the several interpretations contributing to the current thinking. Her inventory of excavated sites is useful as a quick reference.

The Trans-Pecos area is reviewed by Donald J. Lehmer, now with the University of Omaha and a major contributor to the archaeology of Trans-Pecos Texas. Lehmer's discussion of the past investigations and current interpretations of this area are indeed clear, concrete, and definitive. He bases this discussion on a long-standing and intimate knowledge of the material and careful consideration of all of the data. It provides a major clarification and revision of this section of the "Handbook." He separates the area into four subdivisions, though the fourth is somewhat tenuous, and ties them to a substantial chronology based upon alluvial sequences. Distinctions between the subdivisions in both time and space are made specific and concrete.

Thomas N. Campbell, Chairman of the Department of Anthropology at the University of Texas and one who has worked extensively along the Texas Gulf Coast, has summarized the archaeology of that area in the fifth section. After a good but brief description of this little-known archaeological area Campbell emphasized the urgent need for investigations, resulting from destruction of sites by both natural forces and modern cultural advances in the area. His intimate acquaintance with past work here provides an excellent clarification of the present archaeological picture, poorly-known as it is, and points out explicitly the unsolved problems. These problems largely stem from lack of field work along this coast, as he so carefully emphasizes. Campbell has, unfortunately, adopted the misleading and sterile terms of "Paleo-American" and "Neo-American" devised in the "Handbook."

The final section, also by T. N. Campbell, is a guide to the literature pertaining to Texas archaeology and is a revision and expansion of a similar guide published by him in 1952. In the present version 1287 items are listed by author, almost doubling the entries in the first compilation. In addition, he has grouped these numbered references into an index by regions, an index by counties, an index by cultures, and an index by topics. This is a thorough job and a good one.

In all, this is a highly commendable volume, equally useful to the professional specialist, the amateur collector, and the general student of North American prehistory. I hope a large printing was run because it would make a useful text for the Texas portion of a course in North American prehistory.

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An Introduction to Plains Apache Archeology: The Dismal River Aspect. James H. Gunnerson. Anthropological Papers, No. 58, Bureau of American Ethnology, Bulletin 173, pp. 131–160, Washington, 1960. 130 pp., 12 figs., 38 pls.

In this work Gunnerson defines as an Aspect a somewhat obscure archaeological manifestation in the westerly reaches of the central Great Plains. To the non-specialist the badly fragmented Dismal River ceramic remains, the disappointingly incomplete house remains, and the overall "cultural paucity" of the complex might seem to lend little of interest. Yet the list of workers concerned with this material is a roster of the major figures in the development of archaeological research on the Plains: Hill, Strong, Champe, Wedel, and many others. And for good

reason; it turns out that we have here a neat and accurate means of precisely documenting the late phases of the Athabascan occupation of the Plains and Southwest. Certainly the work at hand far outdistances in accuracy and basic soundness other recent and more grandiose attempts to reconstruct the same events by so-called historical methods.

Briefly Gunnerson has utilized his original extensive interest in a single Dismal River Site, the White Cat Village, Harlan County, Nebraska, as a core around which to organize all that he could find out concerning this archaeological manifestation. This information he has then presented in two forms: first, a detailed factual account of archaeological materials recovered from more than 80 sites scattered over four states; secondly, general summaries and conclusions which indicate his ideas not only concerning the basic pattern of this manifestation, but also its relationship to complexes much further afield. The temporal depth is found to be slight, all sites clustering around the opening of the 18th century, but the areal implications reach from the Great Salt Lake to the Rio Grande, and northward to the original Athabascan hearth. It is clear from this work that both the Spanish and French incursions on the western central Plains met virtually nomadic groups of horticulturalists whose hunting activities were extensive and whose contacts extended from central Nebraska to New Mexico. It is also clear that these peoples spoke Apachean dialects, and that we have here detailed information concerning many aspects of the material culture of the Plains Apache. Essentially Gunnerson sees this as being borrowed mainly from late Plains complexes with some few additions from the Southwest. It is not at all certain what these groups brought from their northern home, save their speech habits. The study contains admirably detailed lists of all finds, explanations of subsurface details which led to later interpretations, and other devices to allow the reader to judge for himself. The review of previous work and the bibliography seem complete with the exception of reference to the author's original master's thesis on White Cat Village and related archaeological materials.

This study is a welcome addition to the literature concerning the Plains Indians during the twilight period of their early contact with encroaching European populations. It is also a good example of the techniques developed over the past few decades by the archaeologists that are helpful to the fields of ethnology and history in the illumination of various hazy areas of ethnohistoric research, as well as in the validation, or often disproof, of supposedly factual historical data. It is through such documentation that we will ultimately begin to understand in detail the precise steps by which the indigenous New World cultures were transformed into such hybrids as the elaborate Plains Indian culture which so interested the Americanists of the first few decades of this century. If we couple this information with Swadesh's glottochronological suggestions, and the Athabascan linguistic contributions of Hoijer, together with Kroeber's last comments on the matter, we also can gain insight into the ways in which the various research areas of anthropology contribute to the analysis of the complexities of the growth and development of varieties of cultural experience. That the raw material may be flat to the taste is of small moment.

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Site D1–29, a Rockshelter in Northeastern Oklahoma. JOAN E. FREEMAN. With an appendix on skeletal material by Aaron Elkins. Archives of Archaeology, No. 8, Society for American Archaeology and University of Wisconsin Press, Madison, 1960. xiii + 301 pp., 161 figs., 17 tables on 5 double-sided microcards. $2.00.

Salvage Archaeology in Oklahoma, Vol. 1. JAMES B. SHAEFFER. Papers of the Oklahoma Archaeological Salvage Project, Nos. 8–15. Identifications and morphology by Alice M. Brues, A. L. Buck, Oren E. Evans, Charles C. Carpenter, David E. Kitts, and Carl O. Riggs. Archives of Archaeology, No. 9, Society for American Archaeology and University of Wisconsin Press, Madison, 1960. 217 pp., 84 figs. on 3 double-sided microcards. $1.25.

With the exception of a few preliminary statements pertaining to various Gibson ceremonial centers in the eastern part of the state, one would have been hard pressed prior to 1950 to find published reports dealing with other phases and areas of Oklahoma archaeology. Since that time, the publication of two syntheses, several comparative papers, and a number of detailed site studies has contributed materially to our knowledge of a substantial portion of the archaeological record. The two reports reviewed here (printed on microcards) are welcome additions to this increasing literature.

Freeman's contribution presents an analysis of the archaeological materials recovered from the excavation of a rockshelter in Woodward Hollow in northeastern Oklahoma during 1938. The features consist of ash beds, a few burials, and a pit; the artifacts are discussed under the traditional categories of chipped stone, ground stone, bone, shell, and pottery. In one of the final sections entitled "Stratigraphic Interpretation," most of the artifacts are considered to be representative of either Woodland or Neosho focus complexes, the two major occupations at the site.

This report will be especially useful for those workers who are interested in prehistoric developments in the Ozark region. The discussion of the pottery type, Woodward Plain, is particularly important, since the Neosho variety of this type is used more as a finite time indicator than has been the case in past studies of excavated sites in eastern Oklahoma in which Woodward Plain was simply a convenient catch-all for most plain, shell-tempered pottery. Although the majority of the other artifacts from the rockshelter are assigned to either Woodland or Neosho focus levels, some of the projectile point types and various ground stone and bone tools are common to both occupations. In view of this fact, the report

would have been enhanced by a brief discussion of the relationship between the evidence for the indigenous development of the Neosho focus as opposed to the evidence that links the Neosho focus with Upper Mississippian complexes. Notwithstanding this lack, the author has performed a valuable service in providing a wealth of descriptive data useful for comparative purposes.

Shaeffer's work contains a summary of the procedures that are involved in the conduct of the Highway Salvage Program in Oklahoma, and an analysis of the archaeological materials recovered from eight sites excavated during 1957–58 under the auspices of this program.

The summary of the procedures for the operation of the Highway Salvage Program mentions a number of the problems that confront the salvage archaeologist in planning, surveying, reporting, excavation, and cost accounting. This excellent presentation points up the need for close cooperation and liaison between the salvage archaeologist and the contractor and with highway personnel at both state and local levels.

A review of the prehistoric cultural horizons in Oklahoma forms the background for a discussion of the excavated sites, which are located in the eastern, central, and western portions of the state. Each site is covered by sections on location, discovery, excavation procedure, detailed description of features and artifacts, and identification of lithic sources and osteological remains. Comparative materials are then drawn upon in order to determine the chronological position of each site. Some of the interpretations that result from these comparisons are not convincing, particularly the statements (pp. 200–1) pertaining to pottery at the Hedding site in western Oklahoma in which the plain sherds are considered to be representative of a Gibson ceramic tradition associated with an early occupation, while the cordmarked sherds are thought to have central Plains affiliations and an association with a late occupation. Nevertheless, the author has presented highly competent and informative studies of a series of sites excavated under the pressures of highway construction.

As a final comment we can add that, in terms of general usability, a bound volume is preferable to the microcard format. However, there is little question that the microcard process is an economical medium for publishing accounts of descriptive data that might otherwise remain buried in the files or on the shelves in the storeroom.

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Coal-oil Canyon (14LO1), Report on a Preliminary Investigation. Peter W. Bowman. Bulletin of the Kansas Anthropological Association, Hays, 1960. ii+79 pp., 6 figs., 17 pl. $2.25.

The subject of this report is an interesting multi-component site situated in the Smoky Hill River drainage of western Kansas, some 30 miles from the Colorado line. The archaeological zone is a rocky talus at the head of a gully, where complexly stratified cultural materials occur discontinuously in soil pockets amongst broken masses of the Ogallala mortar beds into which Coal-oil Canyon, like innumerable other gullies along the High Plains margin, is advancing by headward erosion.

From these deposits, the investigators have recovered several restorable pottery vessels, some hundreds of potsherds, projectile points, scrapers, drills, knives, other stone artifacts, and a few bone implements. These are surprisingly varied, and strongly suggest repeated visits to the spot over a period of perhaps a thousand years or more, beginning with Woodland peoples, continuing through Upper Republican and Dismal River times, and ending apparently with visits from the Pawnee in historic times. Most of the remains seem to belong to the Upper Republican and Dismal River traditions. If the recently reported, but still unpublished, occurrence of boatstone fragments from Coal-oil can be duplicated and their cultural associations established, the time span of the earlier occupations may be extended much farther into the past.

The occupations, inferentially, were of temporary nature, since no evidence of habitations or of prolonged residence has yet come to light anywhere in the immediate locality. The author speculates on the objectives which might have drawn the several groups to the locality, such as hunting, the gathering of pigment materials from local ochre deposits, and on the possibility that the site may have had more attractions (water, for example) in the past than are now readily apparent. His tentative conclusions seem entirely appropriate and at no point do they stretch the available evidence. The thorny geologic and topographic problems are handled with skill and insight; and the archaeological data are presented and discussed with a fullness and clarity that measure up to professional standards in all particulars. Like other sites in the High Plains region, Coal-oil Canyon promises no rich harvest of artifacts; but when other archaeological localities that certainly exist along the margins of the shortgrass belt have been equally conscientiously worked and reported, our knowledge of the pre-horse occupants of the region will be very materially advanced.

Coal-oil Canyon was discovered and has been investigated entirely by non-professionals; but the work there has been carried on in a thoroughly professional manner, despite an extraordinarily difficult archaeological situation. Bowman and his associates are to be commended for the high quality of their operations. As the first full scale technical bulletin issued by the Kansas Anthropological Association, one can only express satisfaction about the product and add the hope that other equally competent and scholarly papers may be forthcoming from its members.

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NOTES AND NEWS

EDITED BY NATHALIE F. S. WOODBURY

COUNCIL FOR OLD WORLD ARCHAEOLOGY

The chief purpose of the Council for Old World Archaeology (COWA), a non-profit corporation founded by the late Lauriston Ward, is to aid professional archaeologists, scholars in allied subjects, students, and laymen to keep abreast of the latest activities and publications in the prehistory and archaeology of the Old World.

To achieve this end the Council publishes a biennial series of COWA *Surveys and Bibliographies* comprising twenty-two area reports on the archaeology of the entire Old World from Palaeolithic to recent historical times. The areas are: British Isles, Scandinavia, Western Europe, Western Mediterranean, Central Europe, Balkans, Eastern Mediterranean, European Russia, Northeast Africa, West Africa, Equatorial Africa, South Africa, East Africa, Western Asia, Southern Asia, Far East, Northern Asia, Southeast Asia, Indonesia, Pacific Islands, Australia.

Each area report covers the last two or three years of archaeological activity in the area, and consists of a survey of current work and an annotated bibliography of the more important books and articles. The Editor-in-Chief is Donald Freeman Brown.

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Correspondence should be addressed to: Council for Old World Archaeology, 11 Divinity Avenue, Cambridge 38, Massachusetts.

PLAINS

PLAINS CONFERENCE. The 18th Plains Conference was held at the University of Oklahoma, Norman, November 24–26, 1960, with Joe Ben Wheat as General Chairman. This was a departure from the procedure of the past 13 years during which this conference met annually at the University of Nebraska in Lincoln. Following the morning registration and pre-business meeting, the afternoon session, November 24, was devoted to reports of field work of the 1960 season with Robert L. Stephenson as chairman. The 12 reports covered current work in the northern Plains, including Alberta, North Dakota, South Dakota, Nebraska, and Colorado, a report of current developments in Plains dendrochronology, and an interesting description of laboratory excavation of a complete Woodland type ossuary that had been brought into the laboratory in a plaster jacket. At the Thanksgiving evening annual banquet John L. Champe was given a testimonial for long and devoted service to the Plains Conference upon the occasion of his retirement from the chairmanship of the Department of Anthropology at the University of Nebraska.

A session of 12 more field reports on November 25, chaired by E. Mott Davis, described current work in Texas, Oklahoma, Kansas, Arkansas, and Missouri, and included an interesting report on chain mail and other exotic material from southern Kansas. The necessity of having two report sessions emphasizes the increasing work in Plains archaeology.

Friday afternoon Champe was chairman of a general session on Plains prehistory including Carlyle S. Smith's report on gun flint manufacture in the Cher Valley, France, James H. Howard's discussion of the White Bull manuscript, W. Raymond Wood's analytical treatment of incised Plains pottery, and Robert W. Neuman's description of the Brother-of-All document. The second half of this Friday afternoon session was devoted to a panel discussion of "The Aksarben Aspect," a paper read by Champe. Discussants were J. O. Brew, Wesley R. Hurt, Smith, Albert C. Spaulding, Stephenson, and Waldo R. Wedel.

In the evening the conference enjoyed a panel discussion, moderated by Savoie Lottinville, at the Stovall Museum on "Future Tasks for Anthropology in the Plains." Participants were Wedel, Donald Berthrong, Alice Marriott, Frank Hibben, and Murial Wright.

Saturday morning, November 26, Howard chaired a symposium on the French and Spanish entradas into the Plains. Principal participants were Champe and Albert H. Schroeder. In the afternoon, the business session selected Marvin Tong as the next Chairman and Lawton, Oklahoma, as the site of the next Plains Conference on Thanksgiving weekend, 1961. The final session, following the business meeting was devoted to contributed papers. These were: "A 'Plains Type' Early Man Site in Ecuador," by William J. Mayer-Oakes; "An Illinois Hopewell Site in Kampsville, Illinois," by Gregory Perino; "A Discussion of Antiquity Laws," by Charles McGimsey; "1960 Fieldwork in the Beaver Reservoir, Arkansas," by Hester M. Davis; and "Corpus, Locus, and Tempus," by Champe.

TEXAS. The Texas Archeological Salvage Project had four projects active in the field through the fall and winter of 1960–61. Curtis D. Tunnell had a crew excavating at the Wunderlich site in the Canyon Reservoir area on the Guadalupe River in October. Here two burned rock middens yielded abundant remains of Archaic peoples of the Edwards Plateau aspect. Two time periods are stratigraphically separable at this stie. Lathel F. Duffield had another T.A.S.P. crew excavating at five sites in the McGee Bend Reservoir area on the Angelina River from October to December. The Etoile, Sowell, Blunt, and McElroy sites are small villages of the ceramic period that relate to the Caddoan area immediately to the north, but do not fit into the defined foci. One or more new foci seem to be indicated. The Wolfshead site is a stratified Archaic site with an early component suggested by San Patrice points and one or more later Archaic components. E. Mott Davis and Tunnell surveyed the small

Brushy Creek Reservoir in Fannin County locating eight sites of minor significance during November. Kenneth H. Honea and William A. Davis recorded 24 sites of both ceramic and preceramic occupations in the Somerville Reservoir on Yegua Creek in Washington, Burleson, and Lee counties during January.

Between October and December Davis and Tunnell of the University of Texas, working with a grant from the American Philosophical Society, tested the Harling Mound (or Morgan Mound) on the south bank of Red River in Fannin County. This rectangular, flat-top mound is 100 miles west of any other known flat-top mounds. Structural details of the mound were recorded but artifacts were few, and houses, associated village or other occupational features were not found.

A Texas Memorial Museum project to investigate several 17th and 18th century European settlements along the Gulf Coast has been started by Dee Ann Suhm. These include the Garcitas Creek site in Victoria County, excavated by the Museum in 1950, believed to be La Salle's Fort Saint Louis of the 1680's, and several others.

A survey of all the Texas archaeological materials in the Witte Museum in San Antonio has been undertaken by Mardith K. Schuetz with the hope of establishing a working typology of the lithic tools other than projectile points and analyses of their geographic distribution.

Kansas. The Kansas State Historical Society completed the survey of the John Redmond Reservoir in Coffey County, Kansas. This survey, begun by Roscoe Wilmeth, State Archeologist, in August, was completed by Thomas A. Witty, his successor, in October, 1960, with 36 sites recorded in the flood pool area. Laboratory work by the Society has centered around processing of material from the Wilson Reservoir in Russell County, Kansas, where eight sites were investigated in the summer of 1960. Remodeling of the Society's museum in Topeka has made available three new galleries devoted to military history, agriculture, and Indians, as well as a 200-seat auditorium and an archaeological laboratory, and storage space.

While in France in the summer of 1960, Carlyle S. Smith collected samples of gun flints, and tools used in their manufacture, in the vicinity of Meusnes in the Department of Loir-et-Cher. This is a part of a study to determine characteristics of French as opposed to English gun flints found in collections from archaeological sites in the Plains. Contrary to common belief, all French gun flints are not blond or brown and all English ones are not black. Smith hopes to establish sorting criteria for distinguishing the origins of these specimens.

Nebraska. The dendrochronology section of the Missouri Basin Chronology Program has been accelerated this winter. This program, now in its third year, is attempting to exploit all of the potential resources for chronological studies in building a temporal framework for Plains cultures. Harry E. Weakly, of the U.S. Department of Agriculture, Warren W. Caldwell, of the Smithsonian Institution, and Ward Weakly, a University of Nebraska student, have concentrated their tree-ring studies on a limited area along the Missouri River between Fort Thompson and the Cheyenne River in South Dakota. A master chart has been constructed for this area using oak, ash, and cedar, which extends from the present to A.D. 1520. Archaeological wood, mainly cedar house posts, from a number of sites has been dated by the master chart. Until further checks have been made, release of these dates would be premature. In addition to the master chart, a "floating" sequence of nearly 300 years has been constructed based upon timbers from houses of the Over focus and the Thomas Riggs focus. There is some suggestion that this "floating" chart will match with the master chart but the overlap is too short for dependability. Nevertheless, the tentative dates, using the extended chronology, are quite in line with earlier guess dates. There also appears to be a high degree of consistency between the extended master chart and the long charts that have previously been developed for several areas of western Nebraska.

An agreement implementing highway archaeological survey and salvage has been made between the Nebraska State Department of Roads and the Nebraska State Historical Society. James O. Marshall joined the Historical Society's Museum staff in January as highway salvage archaeologist and has been correlating site records with highway plans. He made brief field trips to highway construction sites during the winter. A new display illustrating various aspects of archaeological salvage has recently been installed in the museum.

South Dakota. Robert W. Neuman, of the Smithsonian Institution, briefly examined two sites in the area of the Gavins Point Dam on the Missouri River in September, 1960. Both were reported by the Corps of Engineers at the dam and proved to be Woodland sites that were being partially destroyed by wave action and bank slumping along the shores of the newly created Lewis and Clark Lake.

In October and November, Neuman and Lee G. Madison of the Smithsonian Institution staff, at the request of the South Dakota Historical Society, spent two weeks in excavations at the Arzberger site (39HU6) in the upper end of the Big Bend Reservoir. Gravel-quarrying operations within this site had exposed a human burial and large midden area. Neuman excavated in the portions of the site now being destroyed and recovered additional material to supplement that excavated from the site in previous years. During this field trip he also took aerial photographs of several of the sites (particularly Buffalo Pasture site) that had been inundated by the Oahe Reservoir and subsequently re-exposed by a lowering of the Oahe water level. On the return to Lincoln, Nebraska, this party examined several sites reported by local collectors near Wessington Springs, South Dakota. The sites represent components of the Anderson and Monroe foci and of Woodland occupations.

The W. H. Over Museum sponsored three small field projects in the fall of 1960. A launch survey of the east banks of the Fort Randall Reservoir was made in cooperation with the National Park Service and the Corps of

Engineers. Robert Gant and José Wilson Rauth of the museum staff located four deeply buried sites. Two of these appear to be Woodland sites, and others are of the nonceramic period.

In September, the museum staff excavated a large bison-kill site (39HD3) near Ree Heights, central South Dakota. Along a gully on the north face of a large terminal moraine, which formed a natural, funnel-shaped bison trap, three bone piles were found that produced whole and often articulated bones. Cut, broken, and whole bones, along with rough choppers were also found at the foot of the gully. Along the upper end of the gully 11 feet of deposit contained bones, charcoal layers, hearths, and artifacts. In the upper 18 inches of this deposit potsherds of the Over focus were found.

A highway salvage operation uncovered a small burial mound along the Big Sioux River south of Canton. The museum staff salvaged burials from the mound fill and from submound pits here. There were both extended and flexed burials as well as partly articulated bundle burials. Nearby, a large Woodland village site was located.

In February 1961, the South Dakota Legislature passed a bill enabling the highway department to enter into agreements for archaeological, geological, and historical salvage.

North Dakota. Preliminary analyses of the 1960 field data recovered by W. Raymond Wood from the Huff site (32MO11), Oahe Reservoir, has been completed at the University of Oregon. As primarily indicated in ceramics and domestic architecture, this large, fortified village is culturally intermediate between the 13th century Thomas Riggs focus and the historic Mandan villages. There are strong suggestions of contacts with the La Roche focus sites of central South Dakota. The three human burials were analyzed by William M. Bass and two of them were found to have 13 thoracic vertebrae.

A University of North Dakota survey party directed by James H. Howard located the site of the Buffalo-Lodge Village, a fortified Plains-Ojibwa village of the 1820's and 1830's. Of interest is the method of locating and identifying the site through the use of landmarks mentioned in Dakota winter count texts and with the aid of old Plains-Ojibwa informants.

Wyoming. Donald J. Lehmer, now at the University of Omaha, and Gerald D. Thompson, are analyzing dry cave material from two rock shelters in the Big Horn Mountains, near Ten Sleep, Wyoming, excavated by George Frison of Ten Sleep. Both collections represent the same cultural complex, that described by Mulloy as "Late Middle Prehistoric." Artifacts include darts, foreshafts, throwing sticks, coiled basketry, and a fur-lined buffalo-hide moccasin. Sandals and the bow are conspicuously absent. A radiocarbon date of A.D. 260 ± 200 has been obtained from charcoal from one of the caves.

The University of Wyoming has been involved in five research projects during 1960. George Agogino directed excavation of Wall Rock Cave, 40 miles north of Laramie, recovering bone, wood, feather, textile, and bark material of late pre-Columbian to early B.C. times. Agogino and E. Galloway, with a crew of University of Wyoming students, reworked the Johnson-Folsom site near La Porte, Colorado. Both projects were supported by the Wenner-Gren Foundation for Anthropological Research. The Hell Gap site, near Hell Gap, Wyoming, was dug by a joint Peabody Museum, Harvard University and University of Wyoming crew with C. and H. Irwin and Agogino as codirectors, and with funds provided by the American Philosophical Society. The U. P. Mammoth site, 35 miles west of Rawlins, was investigated by this same joint party, and at least one mammoth was found to have been killed by man. With funds supplied by the American Academy of Arts and Sciences, Agogino and C. V. Haynes, with student crews, have obtained samples for radiocarbon analyses from the Dent Mammoth site, the Lipscomb Bison site, the Folsom site, several Agate Basin sites, and the Sandia level at the Sandia site. These have been submitted for age determination.

Assembled by Robert L. Stephenson

NORTHERN MISSISSIPPI VALLEY

Dedication of the new Visitor Center at Mound City Group National Monument near Chillicothe, Ohio, May 14, 1961, marks the presentation of the first integrated "on the spot" interpretation by museum exhibits and guided tour of one of the great earthworks of the Hopewellian Indians. The exhibits, which include a selection of the world-famous art objects from the ceremonial offerings accompanying cremations at the necropolis were planned, designed, and executed by the National Park Service Region Five staff and the Washington laboratory of the Museum Branch, in cooperation with Ohio State Museum.

Seven reservoirs now authorized or planned in the Scioto Valley of southern Ohio are being scrutinized by Raymond S. Baby of the Ohio State Museum to determine future salvage needs in the Hopewellian heartland.

Assembled by John L. Cotter

NORTHEAST

New York. With the appointment of Robert Ascher as assistant professor a program in archaeology has been initiated at Cornell University, and is being rapidly developed. The program includes training in archaeological theory and method on both the graduate and undergraduate level. Area emphasis is on the New World, but course work in the Old World is also available. As part of the program a small research and display museum, under Ascher's direction, is being outfitted. Another facet of the program is excavation of northeastern sites in the late spring and early summer months. Graduate training at Cornell is directed to persistent problems in archaeology within the broader frame of anthropology and history.

The Rochester Museum of Arts and Sciences excavated a historic tavern site in Brighton, New York, during July and August, 1960. Alfred K. Guthe and Charles F. Hayes III aided by Daniel M. Barber sought evidence of former outbuildings in the area east of the tavern struc-

ture. This building, now a private residence, is owned by the Society for the Preservation of Landmarks in Western New York. It was built in 1790 by the Stone family. A refuse area and a fence line were located. The refuse yielded earthenware, bone, and other household debris of the 1780–1820 period.

Albert J. Anderson, Staten Island Archeological Society, reports three Clovis-type points have been found on the surface at Rossville, on Staten Island.

Ontario. Excavations in the Donaldson site, a Middle Woodland manifestation on the Saugeen River in the Bruce Peninsula, were conducted by James V. Wright during parts of May and June, 1960. This was a joint National Museum of Canada and University of Toronto project. Two rectangular house floors were cleaned (23 by 17 feet and 17 by 11 feet). Both possessed an interior hearth and interior postmolds. A central line of large postmolds occurred in the larger of the two suggesting the former presence of a gabled roof. Thirteen burials indicate a Walcolid population. A new focus, the Saugeen, has been established on the basis of this site.

From mid-June to the end of August, 1960, Wright carried out an archaeological survey from Sault Ste. Marie west to the Manitoba border following, for the most part, the north shore of Lake Superior and along the Rainy River. Fifty-five sites were located in all and a Middle Woodland site up the Pic River was excavated for a full month. The ceramics of the site are closely connected to the Anderson and Nutimik foci material as defined by MacNeish from southeastern Manitoba. One poorly defined house structure was also located in this site, representing a circular house approximately 12 feet in diameter. Stations of this western expression of Middle Woodland appear to stretch from southeast Manitoba to Manitoulin Island and to share a common ceramic tradition with the more easterly Point Peninsula. Historic Huron sites were found as far west as the mouth of the Michipicoten River on Lake Superior. Late Woodland remains closely related to the Blackduck focus of Minnesota and the Manitoba focus of Manitoba were encountered in abundance as far east as the Pic River and occurred as sporadic sherds even to the Montreal River on Lake Superior. Archaic sites were relatively rare but two caches from an Old Copper grave on Lake Nipigon were recovered. No Early Woodland or paleo-Indian remains were encountered.

Connecticut. A preliminary catalogue of anthropological material at the Stamford Museum and Nature Center has been completed by Bernard W. Powell. Previously uncatalogued collections included both local and exotic specimens. Interesting "finds" made during the work: (1) At least four points in the paleo-Indian traditions. Unfortunately, as with much of the material, exact provenience is lacking, but a continuing search in the Museum's files may disclose additional information relative to these intriguing specimens. Two of the points are broadly Folsomoid; one of the others is like illustrated Clovis points and shows basal grinding. This latter point may be from Missouri; that the others may derive locally is at least a possibility. (2) A Cody knife from a Montana collection. (3) A dozen whole or nearly perfect Peruvian effigy vessels. These are thought to be from the collection of a New England seafaring family. Expert opinion on them is being obtained. (4) Approximately 27,000 drilled shell beads from the Temple Mound at Spiro, Oklahoma. Some interesting ethnological materials are also on hand, including a number of baskets.

Assembled by Alfred K. Guthe

LOWLAND SOUTH AMERICA

Argentina. From January to March, 1960, an expedition from the Instituto de Antropología, Universidad de Córdoba, under the leadership of Alberto Rex Gonzáles investigated archaeological remains, including a series of large monoliths, in western Tucumán province. A preliminary survey made with the aid of aerial photography was followed by excavations in four sites. The results permitted the definition of a new cultural complex called Tafí. Crude pottery, pottery and stone pipes, game pieces, tools of copper and bone, and stone axes are among the characteristic artifacts. Settlements were composed of a ring of stones 10 to 15 m. in diameter surrounded by smaller rings 3 to 6 m. in diameter. Excavation identified the latter as houses and the central ring as a plaza where daily activities took place and where the dead were buried. Large monoliths are associated with the Tafí culture. One was 4.1 m. long; another 3.1 m. long was engraved with a human face. Trade pottery of the Condorhausi type permits the dating of the Tafí culture at between about A.D. 300 and 500.

The museum of the Facultad de Ciencias Naturales de Salta, a dependency of the Universidad Nacional de Tucumán, has formulated an extensive plan to investigate the area between the Río San Francisco and the Sierras Centinela and Maíz Gordo on the west margin of the Chaco in the province of Salta. Except for brief explorations in 1901 by Nordenskiöld and Boman, this area has been relatively neglected. A survey under the direction of Antonio Serrano in 1959 in this area preceded more extensive work during 1960 and 1961.

Assembled by Betty J. Meggers and Alberto Rex González

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4. *A Reappraisal of Peruvian Archaeology.* Assembled by WENDELL C. BENNETT. x+128 pages, 72 figures, 6 tables. 1948. $2.00.
5. *The George C. Davis Site, Cherokee County, Texas.* H. PERRY NEWELL and ALEX D. KRIEGER. xvi+255 pages, 66 figures, 20 tables. 1949. $3.50.
6. *Archaeology of the Columbia-Frazer Region.* MARIAN W. SMITH. viii+46 pages, 4 figures, 3 tables. 1950. Bound and sold with No. 7, $2.50.
7. *Cattle Point: A Stratified Site in the Southern Northwest Coast Region.* ARDEN R. KING. xii+94 pages, 25 figures, 13 tables. 1950. Bound and sold with No. 6, $2.50.
8. *Radiocarbon Dating.* Assembled by FREDERICK JOHNSON. vi+65 pages, 2 figures, 2 tables. 1951. $1.50.
11. *Seminars in Archaeology: 1955.* Edited by ROBERT WAUCHOPE. x+160 pages, 6 figures, 5 tables. 1956. $2.00.
13. *Paracas, Nazca, and Tiahuanacoid Cultural Relationships in South Coastal Peru.* WILLIAM DUNCAN STRONG. vi+48 pages, 18 figures, 4 tables. 1957. $1.00.
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15. *Modern Yucatecan Maya Pottery Making.* RAYMOND H. THOMPSON. x+157 pages, 48 figures. 1958. $2.50.
16. *The Belcher Mound: A Stratified Caddoan Site in Caddo Parish, Louisiana.* CLARENCE H. WEBB. xiv+212 pages, 142 figures, 4 tables. 1959. $3.00.
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A cumulative index of AMERICAN ANTIQUITY, Vols. 1–25, *Memoirs* 1–16, and the *Notebook* is in preparation.

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